THE

PHILOSOPHICAL SIGNIFICANCE

OF

QUINE'S D-THESIS

THE PHILOSOPHICAL SIGNIFICANCE OF QUINE'S D-THESIS

BY

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I hereby declare that I am the sole author of this thesis.

I authorize the University of Waterloo to lend it to other institutions or individuals for the purpose of scholarly research.

Signature ____

Bradley R. Munro

FRONTISPIECE

"I take it that the intent of science is to ease human existence. If you give way to coercion, science can be crippled, and your new machines may simply suggest new drudgeries."

(B. Brecht, ***Galileo***, pp. 123-4)

"It is impossible to leave outside the laboratory door the theory that we wish to test, for without theory it is impossible to regulate a single instrument or interpret a single reading."

(P. Duhem, ***The Aim and Structure of Physical Theory***. p. 182)

"All the sciences are interconnected as by a chain; no one of them can be completely grasped without the others following of themselves and so without taking in the whole encyclopedia at one and the same time."

(R. Descartes, ***Opuscules de 1619-21***, iv, Adam & Tannery vol. x, p. 255.)

"Theories are built upon facts; and conversely the reports upon facts are shot through and through with theoretical interpretation."

(A.N. Whitehead, ***Adventures of Ideas***, Introduction}

"A man demonstrates his rationality, not by a commitment to fixed ideas, stereotyped procedures, or immutable concepts, but by the manner in which, and the occasions on which, he changes those ideas, procedures, and concepts."

(S. Toulmin, ***Human Understanding***, Frontispiece)

"The heart of recent arguments about conceptual change is the insight that no single ideal of 'explanation', or rational justification - such as Plato and Descartes found in formal geometry - is applicable universally in all sciences at all times."

(S. Toulmin, ***Human Understanding***, p. 176)

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PREFACE

With recently sharpened tools, philosophers within the British and American tradition are mounting a renewed attack[1] on the old problem of rationality. We have chosen to enter into a discussion of the problem by way of Quine's D-theoretic remarks in "Two Dogmas of Empiricism".[2] There could have been other points of departure, but it is a matter of personal history that these remarks were the ones that were chosen.

The fundamental problem of rationality covers a vast amount of territory and cannot be adequately treated in one dissertation; however, one can focus on some of the issues as they relate to Quine's D-thesis.

The D-thesis provides a basis for rethinking the question of how one rationally decides between alternative theories of nature. Quine's approach is to treat the whole system of statements in which a particular statement is couched and not the individual statement by itself as the unit of empirical significance. From this point of view, it does not make sense to talk of the truth or falsity of isolated statements. As Grünbaum puts it: "**no one constituent hypothesis H** can ever be **extricated** from the ever-present web of collateral assumptions so as to be **open** to **separate**

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1 Besides Quine, there are others who are attacking the problem of rationality anew - writers like T.S. Kuhn, P.K. Feyerabend, Mary Hesse, and Stephen Toulmin lead the way. Our work concentrates on the development of Quine's D-thesis, although mention will occasionally be made of some of these others.

2 W.V.O. Quine, "Two Dogmas of Empiricism", ***From a Logical Point of View*** 2nd. ed., (New York, 1963) pp. 20-46.

refutation ... just as no such isolation is achievable for purposes of verification.[3] Further, under such a construal, science is no longer pictured as somehow totally objective and value-free, but rather the most important significance of the D-theoretic view is its recognition of the role that purposes, interests, aims, and values play in the procedure of deciding among alternative theories.

Although it is the D-thesis of Quine that serves as the focal point of the dissertation, it is not intended to be principally a dissertation about Quine or Quine's particular philosophical point of view. Some of Quine's views certainly do serve as the point of departure and a great deal of play is given to Quine's position and his particular development to that position. However, that serves as one means of casting light on the import of the D-thesis. The dissertation is principally about the D-thesis and as such some of the development may go in directions that Quine would never wish to go.

In order to capture the philosophical significance of the D-thesis we have approached it from several angles. Not only do we consider Quine's development, but we also consider it from the point of view of its critics and in defending it from these critics we employ solutions from other philosophers, such as Scheffler. In doing so we make departures from a purely Quineian point of view.

The initial response of some people is to regard the D-thesis as a subjectivist theory; however, to interpret the D-theoretic view of science as being totally subjectivist is mistaken. To

[3] A. Grünbaum, ***Philosophical Problems of Space and Time*** (New York,1963), p.108.

treat science as such would be to deny that one could carry on the scientific enterprise of proposing, evaluating, and comparing competing scientific theories. Science has its objective aspects as well as its subjective aspects. The objective basis for science is created by the community of scientists. This basis is continually in a state of flux as new and better discoveries are made through the process of science. Usually the change that takes place in science is slow enough to give the appearance of science as reasonably stable. In spite of this, scientists are like Neurath's ship builder who must rebuild his ship while keeping it afloat at sea:

> Imagine sailors who, far out at sea, transform the shape of their clumsy vessel from a more circular to a more fishlike one. They make use of some drifting timber, besides the timber of the old structure, to modify the skeleton and the hull of their vessel. But they cannot put the ship in dock in order to start from scratch. During their work they stay on the old structure and deal with heavy gales and thundering waves. In transforming their ship they take care that dangerous leakages do not occur. A new ship grows out of the old one, step by step - and while they are still building, the sailors may already be thinking of a new structure, and they will not always agree with one another. The whole business will go on in a way we cannot even anticipate today.[4]

Scientists, like the rest of us, work within a specific socio-cultural milieu. This communal heritage serves as the old ship upon which the modifications are continually being made. We must continue to use it to stave off the gales and waves as we carry out our innovative alterations.

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[4] Otto Neurath, ***Foundations of the Social Sciences*** International Encyclopedia of Unified Science II no. 1 (Chicago, 1944) p. 47.

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ABSTRACT

The philosophical significance of Quine's D-thesis is considered. The D-thesis - "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system? (W.V.0. Quine, ***From a Logical Point of View***, p. 43) - named for Pierre Duhem, marked a significant turning point in the evolution of Quine's thoughts on epistemology.

A brief survey of Quine's epistemological work is presented to show the evolution of his thought.

Adolf Grünbaum's criticisms are examined and it is shown that the D-thesis can be defended from Grünbaum's attacks. Writings of Harry Frankfurt and Philip Quinn are considered in this context. Using some ideas due to Israel Scheffler, we are able to show that a D-theorist can avoid the subjectivist's problems.

We conclude that the D-thesis is worthy of consideration as a possible basis for a philosophy of science.

CHAPTER ONE

Introduction

It has become a cliche to talk about twentieth century man's failure to control his society and his environment. In spite of his tremendous technological success in many areas, man still has an enormous number of difficulties to overcome. The state of North American society of the seventies, particularly in the large urban areas, is indicative of man's failure to cope with both nature and himself. This failure is world-wide. We do not have to go into details about the violent ways in which men in the seventies attempt to 'right' various 'wrongs', but one thinks of terrorist bombings in the Middle East, Ireland and other parts of the world, of wholesale expulsions of peoples at the whim of a despot, of wars of liberation, and so on. Also there is no reason for going into detail about the way that man has misused the Spaceship Earth[5] but one thinks of concrete jungles, pollution of both air and water, the wasteful consumption of resources, and the use of ecological warfare to gain political ends. Enough has been said and written about these matters already. Responsibility for the misuse of scientific 'discoveries' must ultimately rest with the scientist as well as the politician. Too many scientists 'copout' with the excuse that science is somehow value-free and the belief that value questions do not enter into the exercise of an objective science. This notion that science in itself need not take into account the interests and goals of man has led in the end to the misuse of the 'advances' of science. For too long we have suffered under the nineteenth century delusion of a scientist standing aloof from the world of men, confined to some isolated laboratory where he impartially conducts his search for nature's objective facts.

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[5] R. Buckminster Fuller, ***Operating Manual for Spaceship Earth*** (New York, 1972).

Some philosophers attempt to place the question of value in the area of pre-science, that is, prior to the scientific investigation when one is deciding what to study, or they try to place it in the area of post-science, that is, after the investigation has been concluded and one decides how to apply, the scientific discoveries. To do so provides a distorted picture of the scientific enterprise for things rarely if ever neatly separate themselves out so that one can clearly say that such and such an aspect is scientific and so and so is a pre-scientific factor. If one is to characterize science fully, then all the factors that contribute to making science what it is need to be considered. The artificial ploy of defining value out of science in order to preserve some notion of it as value-free is to distort and mislead. When the philosopher of science announces to the world that science is value-free, he misleads his readers for they do not think in terms of science as distinct from pre-science or post-science, but they think in terms of science as a whole which includes the pre-scientific aspects and the post-scientific aspects. Unfortunately for mankind, too many have taken the philosopher at his word; perhaps, because it is comforting to know that one is not to be held responsible for one's actions.[6]

A good philosophy of science should provide a complete picture of the scientific enterprise, and this means providing an account of all the crucial factors that enter into the scientific endeavour.

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6 This method of avoiding responsibility (by defining the responsibility outside of one's own sphere of action) is not peculiar to the scientist. It is rather the institutional disease of the Western world. Corporations and government. agencies, etc. arc often designed with the end of shifting the blame away from the decision-makers of those bodies. For example, in bankruptcy cases, the blame falls upon the corporation and not the individual officers and directors who made the decisions, - so long as the executives have remained within the law (also designed in the interests of corporate leaders and lawyers) their personal assets go untouched. In government agencies, for example, the responsibility for decisions does not reach back to those who ultimately make the decisions., - one has only to think of the Calley case in the United States.

Some account must be provided of the role played by the so-called pre-scientific decisions which do determine the final outcome of the scientific process. In addition, some account must be taken of the so-called post-scientific decisions which determine the applications of a particular discovery and which ultimately determine the future course of the scientific endeavour. Strange as it may seem, reasons of efficiency, the availability of resources both physical and temporal may figure in the decision of what logic or mathematics to use, what assumptions to make, and so on.

Recent literature in the philosophy of science seems to indicate that the subject is undergoing a transition period. A new epistemological basis for science is developing. Fundamentally, this development involves a rejection of the old view of science as somehow totally objective and value free. Questions of fact and value cannot be separated as some Logical Positivists thought. The old paradigm of science is being rejected and a new view of science is gaining hold. No longer is hypothesis-testing in science considered as being somehow divorced from the aims and purposes of the scientists conducting the tests. The picture of hypothesis-testing is seen to be more complicated than was previously supposed.

A not inconsiderable influence comes from the quite radical proposal put forward by W.V.O. Quine. He holds the thesis that: "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system."[7] On the basis of this assumption, hypothesis-testing cannot be construed as merely matching a given hypothesis against nature, but rather it is a matching

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[7] W.V.O. Quine, "Two Dogmas of Empiricism" ***From a Logical Point of View*** (New York, 1963) p. 43.

against nature of the hypothesis **and** all the auxiliary assumptions of the system in which the hypothesis is couched. This means that when a given prediction fails, it is a matter of choice where one places the blame. The hypothesis can be blamed or the hypothesis can be retained in form and the blame placed elsewhere in the system on one or more of the assumptions. Pragmatic considerations enter into the decision of what to preserve and what to alter in a given situation.

Since this matter is so involved and complex, we cannot consider the matter in full in this dissertation. Therefore, as a way of breaking into the complexity gradually and as a way of providing the reader with some idea of the complexity involved, we confine ourselves to a consideration of Quine's thesis and the discussion that has developed in the literature in response to Quine's thesis. We will try to show that the thesis, called the D-thesis, can withstand the criticisms that have been raised thus far. It is our belief that the D-thesis provides the best epistemological basis for science that has been proposed up to now.

Quine's D-thesis

In December 1950, Quine presented a paper to the Eastern Division of the American Philosophical Association at Toronto, Ontario. In that paper, entitled “Two Dogmas of Empiricism”,[8] he presented his field theory of knowledge. This constituted a significant turning-point, in the direction of Quine's philosophical work. Up to that point he was perplexed by a number of epistemological and ontological problems. The thesis presented in the latter part of the “Two

[8] W.V.O. Quine, "Two Dogmas of Empiricism", ***Philosophical Review*** vol. 50 (January, 1951) pp. 20-43; reprinted with minor revisions in ***From a Logical Point of View*** (New York, 1963) pp. 20-46. I will use the version from the book ***From a Logical Point of View***, unless I indicate otherwise.

Dogmas..." paper signalled the beginnings of an approach toward the solution of some of these epistemological and ontological difficulties. Quine followed up this approach in his succeeding writings. He considered his book ***Word and Object***[9] (1960) to be "largely concerned with expanding, supplementing and improving the doctrine that was so inadequately sketched in those last four pages of "Two Dogmas"".[10]

The key thesis of those last four pages is given in Quine's statement that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system".[11] Grünbaum called it the 'D-thesis'[12] because it was similar to a thesis expressed by the French physicist, Pierre Duhem, in the book ***The Aim and Structure of Physical Theory***[13] (1906).

The truth or falsity of a statement, according to the D-thesis, is a function of the system of statements in which it is found. As such when a given statement is apparently falsified it may be preserved by

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9 W.V.O. Quine, ***Word and Object*** (Cambridge, Mass., 1960).

10 From a letter to Professor Harry Frankfurt dated October 2, 1962. See footnote 13 in Chapter IV.

11 W.V.O. Quine, "Two Dogmas...", p. 43.

12 Grünbaum gave the name D-thesis to Quine's thesis in the fourth chapter of his book ***Philosophical Problems of Space and Time*** (New York, 1963) p. 108. Presumably, he had noticed the reference to Duhem in the "Two Dogmas..." paper. In that paper, Quine has a footnote in which he tells us that the thesis "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" has been "well argued by Duhem." ("Two Dogmas..." p. 41) As we shall see Quine's thesis goes much further than the original Duhemian thesis.

13 Pierre Duhem, ***The Aim and Structure of Physical Theory*** (Princeton, 1954) transl. P.P. Wiener. See Appendix one for a discussion of Duhem's thesis. Duhem's book was originally published in French under the title, ***La Théorie Physique: Son Objet, Sa Structure*** 2nd ed. (Paris, 1914), lst ed. (Paris, 1906).

making adjustments elsewhere in the total system. This is a result of the complex meaning connections between the statements. A statement is not meaningful in isolation from the system of statements, but rather acquires its meaning from the way that the concepts are employed within the linguistic context of the system.

Quine wrote:

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reëvaluation of some statements entails reëvaluation of others, because of their logical interconnections --the logical laws being in turn simply certain further statements of the system, certain further elements of the field. Having reëvaluated one statement we must reëvaluate some others, which may be statements logically connected with the first or may be the statements of logical connections themselves. But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to re-ëvaluate in the light of any single contrary experience? No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole.[14]

We call this Quine's **field theory of knowledge**, since he compares ‘total science’ to a field of force. Behind this theory is Quine’s rejection of the statement as the unit of empirical significance. Basically this is a rejection of the Logical Positivists’ general program of verification and, in particular, of Rudolf Carnap’s program of radical reductionism. Quine holds that “the unit

[14] Quine, "Two Dogmas...", pp. 42-3.

of empirical significance is the whole of science."[15] He believes that just as Locke and Hume drew the grid too finely by taking the term to be the unit of empirical significance, so also Frege and Carnap drew the grid too finely when they considered the statement to be the unit of empirical significance.

It is not individual statements that meet experience head on, but rather systems of statements. Systems of statements are true or false of experience; and statements by themselves are not. Should we wish to preserve a given statement within the set of statements to be true in the face of some recalcitrant experience, then we can do so by placing the blame on some other statement or statements in the system and make the appropriate alterations in them so that in the revised system of statements our given statement is true.

Quine's D-thesis has raised a certain amount of controversy. In the third chapter, we will consider some of the literature of the D-thesis controversy. Throughout the dissertation we will be concerned with the meaning of Quine's doctrine and will attempt to provide a meaningful interpretation of the D-thesis that can serve as an epistemological foundation for science.[16]

This chapter and the next chapter are expository. In this chapter we shall briefly acquaint the reader with what Quine has said about the D-thesis in "Two Dogmas of Empiricism". In order to assist the

[15] ***Ibid***., p. 42.

[16] It is our hope that a D-theoretic basis will provide a much firmer foundation for science than the verification principle did in the logical positivist variety of epistemology.

reader to understand Quine's D-theoretic stand, in Chapter Two we will consider the development of Quine's thought as it relates to the D-thesis. (The reader may wish to skip over that chapter and return to it after he has read the rest of the dissertation).

Exposition:

Perhaps the best way to approach the task of exposition is to attempt to indicate what the D-thesis means for the scientific endeavour. A good number of scientists and philosophers hold or have held the belief that there is an asymmetry between the verification of a theory and the falsification of a theory. David Hume and others have pointed out that we cannot prove our scientific theories to be true. Verification is inconclusive. No matter how highly confirmed a theory may be, it is always subject to disconfirmation. In the future something could arise that conflicts with the theory. Since we do not have access to the events of the future, this forms one reason for considering verification to be inconclusive. Another reason is that our theories are not deducible from the experimental data that follow from those theories. In other words, particular experimental results may support many alternative theories and they do not provide us with any way of choosing which of the alternatives is the correct one. Two quite distinct theories may both predict the same results and the results will not indicate which theory is to be preferred. No number of confirmations can fully verify a theory.

Falsification, on the other hand, is considered to be conclusive by those who hold the asymmetry view. It is believed that theories can be conclusively refuted. Since experience cannot decisively pronounce on which alternative theory in a set of competing alternatives is true, there is yet a hope that science can progress by eliminating false theories. Hence, this notion that falsification is conclusive provides the assurance that there can be progress in science. Progress is achieved by a process of elimination. One gradually eliminates those theories that are falsified in experience. One removes those theories that make predictions that do not obtain. Presumably, the true theories will remain unfalsified.

When a scientist is faced with an event that falsifies his theory, he could do one of two things. Either he could revise his theory in which case he has a similar but modified theory or he could abandon the old theory altogether and search for a completely new theory that is more adequate. In either of these cases, it was thought that science would progress by eliminating those theories that are falsified by experiments and retaining those theories that are highly confirmed and as yet unfalsified by experience.

Quine's D-thesis comes to the fore when he considers the verification theory of meaning and Carnap's theory of radical reductionism. We should mention here that Quine's discussion takes place within the context of his investigation of the empiricist dogma of the distinction that exists between analytic and synthetic statements. (The other dogma in the "Two Dogmas of Empiricism" paper is the dogma of reduction.) Quine considered the verification theory of meaning to be relevant to the discussion of the analytic-synthetic distinction because it could be used to obtain a definition of synonymy or sameness of meaning which in turn could be used to provide a definition of analyticity. Quine considered the verification theory of meaning to be the theory that "the meaning of a statement is the method of empirically confirming or infirming it."[17] It could be used to obtain a definition of synonymy or sameness of meaning in the following ways: "what the verification theory says is that statements are synonymous if and only if they are alike in point of method of empirical confirmation or infirmation."[18] Using this account of cognitive synonymy of statements one could define analyticity. In the first part of the "Two Dogmas" paper, Quine recognized two classes of analytic statements. First, those that are logically true and, second, those statements that "can be turned into a logical truth by putting synonyms for synonyms."[19] Hence, the verification theory of meaning directly affects the latter type of analytic statement.

This does not, however, provide us with an easy solution to the problem of analyticity, since there are problems with the verification theory of meaning. Specifically, Quine saw that problems arise over "the nature of the relation between a statement and the experiences which contribute to or detract from its confirmation."[20] (It is here that we find the connection between the two dogmas; reductionism and analytic-synthetic). What Quine calls **radical reductionism** views the relation as one of direct report. According to Quine it is the view that: "Every meaningful statement is held to be translatable into a statement (true or false) about immediate experience."[21] In his book ***Der***

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17 Quine, "Two Dogmas...", p. 37.

18 ***Ibid***.

19 ***Ibid***., p. 23.

20 ***Ibid***., p. 38.

21 ***Ibid***.

Logische Aufbau der Welt[22], Rudolf Carnap made an attempt to carry out a program of radical reduction. He later abandoned the attempt.[23]

According to Quine, Carnap did not begin his task of reducing significant discourse to statements of a sense-datum language with a narrow sense-datum language, but rather he used a language that "included the notations of logic, up through higher set theory."[24] Thus, in addition to sensory events, its ontology included classes, classes of classes, etc. Considering the task to be performed, this in itself seems rather strange. Quine admits that Carnap's constructions were ingenious and suggestive, but he complained that Carnap's "treatment of physical objects fell short of reduction not merely through sketchiness, but in principle"[25]. The problem arose because he tried to assign sense qualities to spatio-temporal point-instants. He gave no indication of how the statements of the form "Quality g is at point-instant x;y;z;t"[26], which were to be used in his construction, were to be translated into his initial language of sense data and logic. As Quine puts it: "The connective "is at" remains an added undefined connective; the canons counsel us in its use but not in its elimination."[27]

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22 R. Carnap, ***The Logical Structure of the World and Pseudo-problems in Philosophy***, transl. R.A. George (Berkeley and Los Angeles, 1967).

23 Quine, "Two Dogmas..." ***op. cit***. p. 40

24 ***Ibid***., p. 39.

25 ***Ibid***., p. 40.

26 ***Ibid***.

27 ***Ibid***.

We must point out here that Quine can only claim failure in principle of Carnap's specific attempt at reduction. He does not show that every approach at reduction is in principle doomed to failure. There may very well be some technique different from Carnap's which works. We do not wish to pursue this question here, for it may also be the case that there is no such possible reduction technique. At present, we have no way of deciding the question either way. It is sufficient to point out that there is some doubt that such a reduction can take place and until someone actually comes up with a reduction that works the issue will remain in limbo. Thus it is something that a good philosopher would wish to avoid as a basis for a philosophical system. From a D-theoretic point of view, of course, we would have to maintain a sceptical stance on this question.

Even though Carnap, the chief exponent of reductionism, seemed to abandon the program in his later writings, the dogma of reductionism remained a part of empiricist philosophy. As Quine puts it:

> The notion lingers that to each statement or each synthetic statement, there is associated a unique range of possible sensory events such as the occurrence of any of them would add to the likelihood of truth of the statement, and that there is associated also another unique range of possible sensory events whose occurrence would detract from that likelihood.[28]

It is precisely this that Quine wishes to deny. Quine holds a position that is counter to this. He believes that it is not the case that each statement can be individually confirmed or infirmed, but rather "our statements about the external world face the tribunal of sense experience not individually

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[28] ***Ibid***., pp. 40-1.

but only as a corporate body."[29] The D-thesis not only denies the reductionist basis for the verification theory of meaning, but because the reductionist's dogma can be used to support the dogma of the distinction in kind between analytic and synthetic statements, it also results in weakening the basis for that distinction.

As we have already indicated, Quine's thesis runs counter to the Logical Positivists' program. Quine does not consider the statement to be the unit of empirical significance, but rather he believes that "the unit of empirical significance is the whole of science."[30] This pronouncement sounds very peculiar, especially in the philosophical milieu that has grown up around the positivist's view of science. To be of any use, this doctrine is in need of elaboration and explanation. How could the unit of empirical significance be the whole of science? What could he mean by the phrase 'the whole of science'? One wonders how one could even determine the empirical significance of anything if one always had to go to the whole of science in order to test it.[31]

To return briefly to our earlier comments on the asymmetry thesis and scientific progress, we must say that Quine's D-thesis also runs counter to that view. From the D-theoretic point of view not only is verification inconclusive, but falsification is inconclusive as well. No statement is conclusively falsifiable, since it can be saved by altering the system of scientific statements in the

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29 ***Ibid***., p. 41.

30 ***Ibid***., p. 42.

31 Of course, if pressed a scientist could probably give an investigator a good idea of his background theory and system of science. A given scientist is probably not aware of all the assumptions that he makes, but a complete analysis of his statements should be able to draw these out.

appropriate way. This, too, is very peculiar and disturbing to anyone brought up within the philosophical milieu that has grown up around the positivist's view of science. It seems to undercut the basis for progress in science, namely the process of eliminating false hypotheses. From the basis of the D-thesis, it would seem that a scientist may preserve his pet theories come what may. We will return to this question later on.

We have called Quine's D-theoretic doctrine, **the field theory of knowledge** because he compared total science to a field of force that is bounded by experience. We have to interpret this as a theory about organized knowledge or systematized knowledge or knowledge as it is found in areas of endeavour like geography, history, physics, chemistry, mathematics, logic, philosophy, and so on. Also when he speaks of total science or the whole of science we should take him to mean a total scientific theory or a whole scientific theory, for we would be hard pressed to find any place where the whole of science (or all the statements of science) has been collected. Also we would be hard pressed to find any collection of scientific statements upon which we could get every scientist to agree. Thus we must interpret him to be talking about entire scientific theories or total scientific systems (whether articulated or not). Perhaps, one might be able to argue that there is an entirely different one for each scientist. It is also probably the case that each scientist holds an evolving theory or one that changes with each 'advance' in science. In addition, it is possible that an individual scientist is not fully aware of all that his theory entails and hence is not aware of his total scientific theory.

Obviously, Quine uses the metaphor of a field of force to indicate the total connectedness of all the various aspects of our intellectual endeavours. Even though we try to slice things up into particular subject areas, Quine is indicating that this is an artificial division and the collection forms one large field. If we follow the analogy of the field, we can think of a farmer who divides his field into areas so that he can plant tomatoes in one part and corn in another and so on. The division is artificial and determined according to the farmer's purposes and convenience. He is restricted, of course, by the boundaries of the field. One year he may have tomatoes on the periphery and another he may have corn or another year each may be on different parts of the periphery. I do not know how far to push this metaphor, but we should note Quine's other metaphor that of science as a man-made fabric. Here, I think the emphasis is on the inter-woven or inter-connected nature of the various subject areas, whereas in the field metaphor, I think the emphasis is really on the boundary conditions and the non-specific way in which the subject matter is connected with experience. That is, it is an artificial question about what is on the periphery.

Let us remove ourselves from the metaphors in order not to be misled by the wrong features and emphasize the other points that Quine makes in a non-metaphorical way. First, he makes the point that the statements of 'total science' are logically interconnected. If one reëvaluates one statement, then one has also to reëvaluate other statements - namely, the ones that are logically connected to the first. Second, there are no clearly defined empirical statements, for "no particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole."[32] Although, as we shall see later on, Quine holds that there are some statements, 'observation statements', that we choose to tie

[32] ***Ibid***., p. 43.

closely to experience, we really have a great deal of choice in determining which statements to alter in the face of contrary experience. Not only can any statement be held true come what may, if we make drastic enough adjustments elsewhere in the system, but also “no statement is immune to revision.”[33]

Given Quine’s position, then there is no ultimate distinction in kind between statements. Given a specific language, one could, of course, specify a certain set of statements that one wishes to hold true come what may and in this way specify a distinction in kind to hold for that language. But this is not to say that there is some set of statements that are always immune from revision for they are only immune from revision so long as the users of that language choose to keep them immune. There is no distinction in kind of statements that stands up under all adjustments. Any distinction is subject to change by altering the language in the appropriate way. Quine, thus, denies that there is some ultimate difference in kind between analytic and synthetic.

The fact that some statements appear to be closer to sense experience than others indicates not a relationship between those statements and experience, but rather indicates “our choosing one statement rather than another for revision in the event of recalcitrant experience.”[34] Given any recalcitrant experience there are any number of ways in which one may alter one's system to account for this experience. According to Quine, our natural tendency is “to disturb the total system as little

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[33] ***Ibid***.

[34] ***Ibid***.

as possible."[35]

Thus those statements which have fewer logical interconnections with other statements are the ones that are probably changed first. Hence, these statements seem to be closer to experience, that is, they seem to be more empirical, - than those statements which we would choose to change last, what Quine calls "the highly theoretical statements of physics or logic or ontology."[36]

One of the matters that bears close attention is this distinction between the more empirical statements and the highly theoretical ones. What constitutes the difference? Why do we choose to alter the latter ones last? Must we alter the latter ones only as a last resort?

Are there times when one would be advised to alter the "highly theoretical statements" in one's system before touching the "more empirical" ones? We will attempt to deal with these and related matters at a later point in the thesis.

The important point to consider here is that Quine's D-thesis runs counter to the Logical Positivists' dogmas of reduction and analytic-synthetic. First, individual statements do not meet the external world in isolation. Second, because of the systematic connections of any given statement, it may be preserved or revised at the discretion of the users. There are no statements that are always immune from revision in the face of experience, nor are here any statements that must always be

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[35] ***Ibid***., p. 44.

[36] ***Ibid***.

revised first in the face of recalcitrant experience. One problem is to determine the reasons for choosing to preserve certain statements under certain circumstances and for choosing to revise others.

Quine's theory not only has implications for truth and meaning, but it also has implications for questions of ontology. At least from the time of Plato, ontological concerns have been intimately tied to epistemology. Likewise, in Quine's view, one's epistemological basis affects one's ontological basis. In his view, physical objects differ from fictional objects in degree and not in kind.

> Physical objects are conceptually imported into the situation as convenient intermediaries - not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer.[37]

Now the full import of Quine's claim is not apparent in the "Two Dogmas" paper. One has to survey Quine's previous writings on the matter of ontology in order to ferret out the point he is making here. (We attempt to do this in Chapter Two). For now we can say that Quine considers physical objects and fictional objects as functioning epistemologically as convenient intermediaries or as devices "for working a manageable structure into the flux of experience."[38] He claims that the reason that we hold tenaciously to the myth of physical objects is because it has helped us to cope

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37 ***Ibid***.

38 ***Ibid***.

more effectively with experience than other myths. Physical objects are not the only things that we posit in order to develop a manageable structure for dealing with the flux of experience, we also posit objects at the atomic level in order to simplify our theories. Forces, abstract entities, classes, classes of classes, and so on, all are posits which differ only according to "the degree to which they expedite our dealings with sense experience."[39]

Conceptual schemes and their attendant ontologies are tools to help us cope with experience. How one chooses a particular conceptual scheme is dependent on a number of factors. From an empiricist point of view, the over-all system must square with experience. Internally, though, the factors determining the structure of the system are non-empirical. The internal structure of the system is where we find the myths and fictions. Which particular myths and fictions are chosen Quine tells us is often a matter of simplicity. Other factors that figure in our choice of conceptual scheme are the efficacy with which it enables us to deal with experience, convenience, elegance, conceptual economy, psychological manageability, conservatism and efficacy in communication. These are essentially pragmatic criteria. These are important factors since they do function in our choices of theoretical structures. Too much emphasis has been placed on the role of the empirical factor and not enough emphasis has been placed on these latter aspects. Quine's thesis can be seen as an attempt to draw attention to this imbalance in our view of the scientific process. The data can support many alternative theories and so empirical evidence is not sufficient for the determination of which theory to prefer. Quine's D-thesis draws attention to the other factors that function in our determinations. Unfortunately, Quine does not expand on these additional factors, but a full working

[39] ***Ibid***.

out of the implications of the D-thesis would have to develop their functions in the determination of theories.

So far, then, we have a brief exposition of Quine's position in the latter parts of the "Two Dogmas" paper. His field theory of knowledge is a peculiar yet all-encompassing doctrine. What would lead him to adopt such a position and where does it lead?

In this thesis, we conduct an examination of Quine's D-thesis and its implications. First, we consider what led him to such a position. Then we will see that it can be defended against the criticism that has been raised so far.

CHAPTER TWO

Quine's Development

and the D-thesis

One of the first questions that springs to mind when one examines the field theory of knowledge presented at the end of the "Two Dogmas"[40] paper is: "How was Quine led to adopt such a position?" In this chapter, we propose to follow Quine's philosophical development to see how he came upon his D-theoretic point of view. We can do this fairly easily by examining Quine's published work. Such an examination reveals that "Two Dogmas of Empiricism" marks a significant turning-point in Quine's philosophical development. The field theory of knowledge develops out of an insight Quine had after grappling with these problems for many years.

Prior to "Two Dogmas" one finds Quine perplexed by a number of overlapping problems in the areas of Epistemology and Ontology. The D-theoretic insight was to realize that philosophers were attempting to place too fine a web over significance. The unit of empirical significance is not the statement, but rather the system of statements in which a particular statement is embedded. Following "Two Dogmas", Quine actively pursued the development of his theory, and this work culminated with the publication of ***Word and Object*** 1960)[41]. Work published since ***Word and***

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[40] W.V.O. Quine, 'Two Dogmas of Empiricism', ***Philosophical Review*** vol. 50, January, 1951, pp. 20-43; reprinted with minor revisions in ***From a Logical Point of View***, (New York, 1963) pp. 20-46. I will use the version from the book ***From a Logical Point of View*** unless indicated otherwise.

[41] W.V.O. Quine, ***Word and Object*** (Cambridge, Mass., 1960).

Object by Quine in the areas of epistemology and ontology is a continuation of concerns developed in ***Word and Object***.

Our task in this Chapter is primarily expository. It is intended to provide the reader with an insight into the sorts of considerations that probably led Quine to his D-theoretic position. This chapter is more about Quine and his particular philosophical development than it is about the D-thesis. The reader may wish to skip over this chapter on first reading and return later.

In Quine's writings one finds at least two conflicting tendencies. On the one hand, there is a metaphysical Quine philosophically working toward a quite liberal D-theoretic metaphysics. On the other hand, there is Quine, the technical logician, concerned with the fine details in the development of an elegant system of logic. This latter Quine presents a more restrictive approach and seems to permit his particular preferences to dictate certain policy decisions (- such as love for desert landscapes - that is, a love for sparse ontologies, etc.) The aim in this chapter is to try to trace the development of the D-theoretic strand as it is spun out. The general trend in the literature seems to give more play to the less liberal Quine, and so at times we have had the feeling that we are running against the received view of his philosophy.

An examination of Quine's early writings reveals that he was perplexed by a certain vagueness in the theory of meaning. As we shall see, he struggles with this for some time, but he does not face it head on until his analysis of the notion of analyticity in "Two Dogmas".

Meaning and Reference

Semantics distinguishes two distinct areas: the theory of meaning and the theory of reference. The theory of meaning deals with the concepts of meaning, synonymy, significance, analyticity and entailment; and the theory of reference deals with the concepts of naming, truth, denotation, and extension.[42]

In his paper "Notes on Existence and Necessity."[43] (1943), he uses Frege's Morning star-Evening star example to show the distinction between meaning and reference. It is clear that the names 'Morning Star' and 'Evening Star' do not have the same meaning. However, astronomical investigations allow that these names refer to the same planet, Venus. Therefore, while the two names do not have the same meaning, they do have the same reference. The question of meaning is thus distinguished from the question of reference. To ascertain the meaning of a term it does not suffice to consider its reference. Further, a word like 'Pegasus' has a certain meaning - it can be described as a winged horse, - but it has no reference. Quine writes: "In particular, substantives such as 'Pegasus' that fail to designate are not without meaning; in fact, it is only with an eye to the meaning of 'Pegasus' that we are able to conclude from a study of zoology that the word does not designate."[44] As we shall see, it is in separating the question of meaning from the question of

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42 W.V.O. Quine,'Semantics and Abstract Objects', ***Proceedings of the American Academy of Arts and Sciences***, 1951 p. 91. This paper was read in Boston at the meeting of the Institute for the Unity of Science in April, 1950. (***From a Logical Point of View***. ***op. cit***., p. 170.)

43 W.V.O. Quine, 'Notes on Existence and Necessity' ***The Journal of Philosophy***, vol. XL, no. 5., (March 4, 1943).

44 ***Ibid***., p. 119.

reference that we avoid the quandary that arises over sentences like “Pegasus does not exist.”[45]

Reference, Ontology, and Language

Reference and ontology are closely tied. In an early (1939) paper entitled ‘A Logistical Approach to the Ontological Problem’,[46] Quine discusses the relationship between naming and ontology. He distinguishes between names and syncategorematic words. A syncategorematic word is a sign “like prepositions, conjunctions, articles, commas, etc.: that though it occurs as an essential part of various meaningful sentences it is not a **name** of anything.”[47] Of course, in order to understand what a syncategorematic word is we need to understand what a name is. Names “are simply those constant expressions which replace variables and are replaced by variables according to the usual laws of quantification.”[48]

Quine uses the distinction to transform ontological questions into linguistic questions.[49]

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[45] ***Ibid***.

[46] W.V.O. Quine, 'A Logical Approach to the Ontological Problem', ***The Ways of Paradox***, (New York, 1966), p. 64. A version of this paper was presented in 1939 to the fifth International Congress for the Unity of Science. Some of the paper was published in the ***Journal of Philosophy*** for December 21, 1939, (vol. XXXVI, no. 26) pp. 64-69, under the title 'Designation and Existence'. Quine wrote that 'Designation and Existence' constituted "the bulk of a paper which was read at the Fifth International Congress for the Unity of Science, Cambridge, Mass., September 9, 1939, under the title 'A Logistical Approach to the Ontological Problem.' (***Journal of Philosophy***, XXXVI, p, 701) He adds that a six-page abstract was to be published in vol. 9 of the ***Journal of Unified Science***. This is the paper in ***The Ways of Paradox***. He explains there that the volume of the ***Journal of Unified Science*** never appeared as a result of World War II.(***The Ways of Paradox***, p. 64).

[47] ***Ibid***., p. 64.

[48] ***Ibid***., p. 66.

[49] This foreshadows his doctrine of 'semantic ascent'.

Names are the words that denote entities, and since syncategorematic expressions do not name anything, they do not denote any entities. In this way ontological questions are shifted to the linguistic level where we decide which words name and which words are syncategorematic. Quine uses the theory of quantification as a way of distinguishing these. “To be is to be the value of a variable.”[50] If the entity in question is included in the range of the variables of a particular language, then for that language the entity is said to be. He writes: “We may be said to countenance such and such an entity if and only if we regard the range of our variables as including such an entity.”[51]

Problems arise over what Quine calls “convenient fictions.”[52] These are found in the situation where “one sometimes chooses to speak **as if** certain syncategorematic expressions were the names of entities, though still holding that this is merely a manner of speaking, that the expressions are not actually names.”[53] Convenient fictions result from extending quantifications definitional. This occurs when one treats syncategorematic expressions as if they were names and thus permits them to replace variables or be replaced by variables according to the laws of quantification. A language that employs convenient fictions can be translated into a language that contains only primitive entities. Thus convenient fictions involve “conventions of notational

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50 Quine, 'A Logistical...', ***op. cit***., p. 66.

He says: "It thus appears suitable to describe **names** simply as those constant expressions which replace variables and are replaced by variables according to the usual laws of quantification." (***Ibid***.)

51 ***Ibid***.

52 ***Ibid***.

53 ***Ibid.***

abbreviation."[54] They are definable in terms of the primitive entities countenanced by the language. Quine writes: "What are fictions, from the point of view of a given language, depends on what positions are accessible to variables definitionally rather than primitively. Shift of language ordinarily involves a shift of ontology."[55]

According to Quine, what entities there are differs from language to language - a language determines its objects. In any given language one should be able to distinguish between primitive names or those words that denote entities and convenient fictions or definitionally introduced 'names'.[56] The problem that arises is how one distinguishes between those words that refer to real entities, the primitive names, and syncategorematic words that act as if they were names, the definitional 'names'. If one is to appeal to the fact that the latter are defined in terms of the former, one can ask: When does one know that one has all the definitional 'names' in the proper relationship with the primitive names? In other words, how does one determine what entities a particular language countenances as its primitive **entities**? This is a question that Quine does not answer in this particular paper.

Quine does point out that there is a sense in which the ontological question transcends what he calls 'linguistic convention'. He writes: "How economical an ontology **can** we achieve and still

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54 ***Ibid***., p. 67.

55 ***Ibid***., p. 68.

56 I put this in quotations in order to indicate that these are not really names but syncategorematic expressions which are treated as if they are names.

have a language adequate to all purposes of science? In this form the question of the ontological presuppositions of science survives."[57] From this point of view, primitive names would be those that referred to the members of this most economical ontology call it the "minimum adequate ontology." Unfortunately Quine does not specifically tell us here how we might be able to determine what such a minimum adequate ontology would be.

In "A Logistical Approach to the Ontological Problem," Quine is probing for a solution to the traditional ontological problem of universals. He begins the paper by wondering what it might mean to ask "whether there is such an entity as roundness."[58] His distinction between names and syncategorematic words is an attempt to show how we can use a word like 'roundness' in a meaningful way without having it refer to some entity. Hence, the above question is not a question about the meaningfulness of the word 'roundness', but rather a question of classification: - Is it a name or a syncategorematic word?

Quine's logistic point is that the logical rules covering existential quantification were intended to capture the meaning of the ordinary "idioms 'there is an entity such that', 'an entity exists such that', etc."[59] Existential quantification can, then, serve as 'a formal basis for distinguishing names from syncategorematic expression.'[60] Thus, a term such as 'roundness' can be taken as a

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[57] Quine, 'A Logistical...', ***op. cit***., p. 68.

[58] ***Ibid***., p. 64.

[59] ***Ibid***., p. 65.

[60] ***Ibid***.

name if it serves in a given language as a constant which can replace a variable or be replaced by a variable according to the laws of quantification.[61]

Quine's logistic point does provide us with a means of centring out the ontology for a given language, but it fails to provide a solution to the ontological problem. Quine's quest for a minimum adequate ontology can be seen as a reflection of his nominalistic tendencies. Yet he offers nothing here to resolve the dispute between the nominalist, who "would like to suppress "universals" - the **classes** of our universe - and keep only the concrete individuals (whatever these may be)",[62] and those who like transcendent universes which consist of "concrete individuals of some sort of other, plus all classes of such entities, plus all classes formed from the thus supplemented totality of entities

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[61] Naturally, Quine restricts this "only to those familiar forms of language in which quantification figures as primitive and variables figure solely as adjuncts to quantification". (***Ibid***., p. 66.) Suitable transformations would be needed to adopt this technique to other kinds of languages - i.e., "Languages in which abstraction is primitive" (***Ibid***.) (see his ***System of Logistic*** (Cambridge, Mass., 1934) and his "Logic Based on inclusion and abstraction", ***Journal of Symbolic Logic*** II (1937) pp. 145-152. - reprinted in his ***Selected Logic Papers*** (New York, 1966) pp. 100-109.), and "languages in which variables are eliminated in favor of combinators" (***Ibid***.) (Schönfinkel, Moses. Über die Bausteine der mathematischen Logik," ***Mathematische Annalen*** 92 (1924) pp. 305-316. -Translated in J. Van Heijenoort, ***From Frege to Gödel***, (Cambridge, Mass., 1967) pp. 355-366. and see Curry, H.B. "Grundlagen der kombinatorischen Logik," ***American Journal of Mathematics*** 52 (1930) pp. 509-536, 789-834. and also Curry, H.B. & Feys, Robert. ***Combinatory Logic***, (Amsterdam: North Holland, 1958).)

[62] Quine, "A Logistical. . ." ***op. cit***. , p. 69

The fact that Quine says in parentheses "whatever these may be" reflects a departure from traditional nominalism. The notion of concrete individual is more general than traditionally construed. Quine, along with Nelson Goodman experimented with nominalism (see N. Goodman and W.V.0. Quine, "Steps Towards a Constructive Nominalism", ***Journal of Symbolic Logic*** XII [1947) pp. 105-122. and W.V.O. Quine, "On Universals" ***Journal of Symbolic Logic*** XII (1947) pp. 74-84.) In "On Universals", he discusses nominalism vs. platonism and attempts to show how the nominalist can have universals or classes. In "Steps Towards a Constructive Nominalism", Goodman and Quine set out the rather strong position that "any system that countenances abstract entities we deem unsatisfactory as a final philosophy." (p. 105) Quine, of course, later repudiates this extreme nominalist position as we see in the text of our Chapter Two.

and so on."[63]

Quine sees nominalism basically as "a protest against a transcendent universe."[64] Such a universe runs against common sense. The nominalist attempts to gain some kind of control over ontology.[65] A transcendentalist, (better, a Platonist) on the other hand, would probably argue that he need not be subject to the restrictions of common sense, for such nominalistic restrictions seem to sacrifice parts of classical mathematics that are essential to science. It would seem that the only way that nominalist could win the dispute is if he could show that those sacrificed parts were not necessary for science; - otherwise the Platonist wins because his language is more adequate for science in general.

Quine's search for a resolution of the ontological controversy leads him towards his D-theoretic stance. We shall attempt to show how that is so in what follows.

In his 1943 paper entitled "Notes on Existence and Necessity",[66] Quine reviews the relationship between designation and existence. He points out that: the notion of existence involved in this relationship is a very broad notion of existence and "does not connote existence in any

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63 Quine, "A Logistical..." ***op. cit***., p. 68.

64 ***Ibid***., p. 69.

65 Quine says: "A transcendental totality is one every combination of whose members determines a further member. Such a universe is worse than infinite: to speak of its cardinal number at all entails revision of the classical infinite arithmetic, since either the number is the highest of all numbers or else parts of the universe have higher cardinal numbers than the whole." (***Ibid***.)

66 Quine, 'Notes...', ***op. cit***.

peculiarly spatial or temporal sense."[67] The existential quantifier, "∃x', can be used to indicate the existence of something in many senses other than in the spatial and temporal sense. For example, it may be used to indicate that there exists a number with certain properties. This number, of course, does not exist in any spatial or temporal sense.

He also points out that the connection between existential quantification and designation is implicit in the inference of existential generalization. He writes: "The idea behind such inference is that whatever is true of the object designated by a given substantive is true of something; and clearly the inference loses its justification when the substantive is question does not happen to designate."[68] Thus one cannot determine the ontology for a particular language just by examining the vocabulary of that language because some substantives can be used meaningfully but still not be used designatively. According to Quine, it is only the designative use of a substantive that commits us to the object designated by the substantive.[69] For example, just because someone

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[67] ***Ibid***., p. 116.

[68] ***Ibid***.

[69] At the beginning of this paper Quine has a long discussion of a notion which he later calls 'referential opacity'. The terms 'referential opacity' or 'referentially opaque' do not occur in the 1943 article but in a later article entitled "Reference and Modality" (in ***From a Logical Point of View***, pp. 139-159.) This latter paper was a fusion of the 1943 article and the 1947 article "The Problem of Interpreting Modal Logic" (***Journal of Symbolic Logic***, vol. 12, (1917) pp. 13-48). He introduces the notion of referential opacity in that paper to apply to the situation developed in the 1913 paper. In the 1943 paper, he depends on a different notion, the notion of 'purely designative'. It is interesting to note that in the later article he leaves out the notion of 'purely designative entirely and changes any use of the words 'purely designative' to the words 'purely referential'. This could be merely a preferential decision since it would appear that he uses the words 'designate' and 'refer' interchangeably.

> Quine used the notion of 'purely designative' to distinguish between situations where a name refers simply to the object designated and a situation where the name does not refer simply to the object designated. The former is a 'purely designative' use and the latter is not. A name refers simply to the object designated whenever "whatever can be affirmed about the

uses the word ‘Pegasus’ that does not commit that person to a claim about the existence of the object designated by the word ‘Pegasus’. The person is only committed to a claim about the existence of the object if he uses the substantive designatively. One determines whether a person is using a substantive designatively by determining what objects he treats as “falling with the subject-matter of his quantifiers - within the range of values of his variables.”[70] Existential generalization and also universal instantiation works only for “the case where a substantive

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object remains true when we refer to the object by any other name." (p. 114, 'Notes...') When one cannot substitute alternative names for the object into the context of a true statement containing that name without making the statement false then we have a situation where a name is not purely designative. He gives some examples to illustrate his point. We shall briefly consider one example here. Take the two statements:

(1) Cicero= Tully
(2) 'Cicero' contains six letters. (p. 113, 'Notes...')

Both statements are true, but if we replace the name 'Cicero' by 'Tully' in the second to make the statement:

(3) 'Tully' contains six letters.

we create a false statement. The name 'Cicero' designates the man Cicero and since the man Cicero was also designated by the name 'Tully', then whatever is said about the man Cicero can be said about the man Tully. However, in the case that we are considering we have a use of 'Cicero' that is not purely designative since we cannot substitute 'Tully' for 'Cicero' in sentence (2) and have a true statement result.

There are other contexts where this situation arises. As Quine puts it, "the contexts 'is unaware that...' and 'believes that...' are, like the context of single quotes, contexts in which names do not occur purely designatively. The same is true of the contexts, 'knows that...', 'says that...', 'doubts that...', 'is surprised that...', etc." (p. 115, 'Notes...') These are all contexts which he later calls 'referentially opaque contexts'. (see p. 142, ***From a Logical Point of View***)

In this article Quine points out that in contexts where modal operators are applied to statements, the same kind of situation results. In his later writings he makes a lot of mileage out of the referential opacity of modal contexts. His claim set up a dispute involving Church, Carnap, and Smullyan over quantification in modal contexts. This dispute is documented somewhat in 'Reference and Modality'.

[70] Quine, 'Notes...', ***op. cit.***, p. 118.

designates, and, furthermore, occurs designatively."[71]

It is certainly the case that: a word like 'Pegasus' which does not designate anything can be used meaningfully. This is where the important distinction between meaning and reference enters. Quine discusses this problem in his 1948 paper "On What There Is".[72] He introduces the paper by depicting two philosophers in dispute over whether a particular entity does or does not exist. One disputant says the entity exists and the other says that it does not exist. We could imagine that the dispute is over the existence of some entity like Pegasus. The one who denies the existence of the entity is, according to Quine, tangled, so to speak, in **Plato's beard** - the old riddle of non-being. He must admit that the entity exists in some sense in order that he may be able to deny its existence. The claim is that one could not coherently deny the existence of Pegasus, for if there were no Pegasus then one would not be talking about anything when one used the word 'Pegasus' and so to deny the existence of Pegasus would be nonsense. The one disputant thus argues that Pegasus must exist since we are not speaking nonsense when we talk about Pegasus. However when he is pressed for details about the sort of existence that Pegasus has confusion sets in. Since he is not willing to admit a space-time (flesh and blood) existence for a flying horse, our disputant begins to talk of mental entities, unactualized possibles and so on. He first talks of Pegasus as an idea in men's minds. However, when people deny the existence of a flying horse, they are not talking about a mental entity. Quine points out that one never confuses the Parthenon, for example, with the

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71 ***Ibid***.

72 W.V.0. Quine, "On What There Is", From a Logical Point of View pp. 1-19.Originally, it appeared in the ***Review of Metaphysics***, 1948.

Parthenon - idea, so why should we make such a shift in the case of Pegasus?

The next tactic is to talk about Pegasus as an unactualized possible. This is to say "that Pegasus does not have the special attribute of actuality."[73] (Just as Pegasus is not red means that it lacks the attribute red.) Quine believes that this is one way of ruining "the good old word 'exist'."[74] On this view, 'existence' is one thing and 'subsistence' is another. It is granted that the entity is not actualized in space and time and hence does not exist in this sense, but the entity has being or subsists. Quine says:

> The only way I know of coping with this obfuscation of issues is to **give** Wyman the word 'exist'. I'll try not to use it again; I still have 'is'.[75]

Quine's other objection, which derives from his nominalistic sympathies, is that it also gives rise to an overpopulated universe. As he puts it: "It offends the aesthetic sense of us who have a taste for desert landscapes."[76] There are also problems about how to talk about possibles, especially with respect to the concept of identity.

Take, for instance, the possible fat man in that doorway; and again, the possible bald man in that doorway. Are they the same possible man, or two possible men? How do we decide? How

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[73] ***Ibid***., p. 3.

[74] ***Ibid***.

[75] ***Ibid***.

[76] ***Ibid***., p. 4

many possible men are there in that doorway? Are there more possible thin ones than fat ones? ...But what sense can be found in talking of entities which cannot meaningfully be said to be identical with themselves and distinct from one another?[77]

Even more problems arise if we talk about unactualized impossibles. Quine wonders whether or not one can have unactualized impossibles as well, such as “the round square cupola on Berkeley College.”[78] One common way out of this kind of situation is to declare such contradictory phrases as meaningless. The problem with this procedure as a way out is that there is no generally applicable test for contradictoriness and hence it is “impossible, in principle, ever to devise an effective test of what is meaningful and what is not.”[79]

Quine points out that Russell’s theory of descriptions showed “clearly how we might meaningfully use seeming names without supposing that there be the entities allegedly named.’[80] A complex descriptive name such as ‘the round square cupola on Berkeley College’ is analyzed by Russell as a fragment of the whole sentence in which it appears. Thus we have the following analysis for the sentence “The round square cupola on Berkeley College is pink”:

“Something is round and square and is a cupola on Berkeley College and is pink and nothing

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[77] ***Ibid***.

[78] ***Ibid***.

[79] ***Ibid***., p. 5. Quine is referring here to Church's paper: "A Note on the Entscheidungsproblem", ***Journal of Symbolic Logic*** I (1936), 40f, 101f.

[80] Quine, "On What...", ***Ibid***., p. 5.

else is round and square and a cupola on Berkeley College."[81]

In this way "the seeming name, a descriptive phrase, is paraphrased **in context** as a so-called incomplete symbol."[82] Now the burden of objective reference which had been incorrectly placed on the descriptive phrase 'the round square cupola on Berkeley College' is "taken over by words of the kind that logicians call bound variables, variables of quantification, namely, words like 'something', 'nothing', 'everything'."[83] The important thing about these words is that they do not purport to be names, but rather "they refer to entities generally, with a kind of studied ambiguity peculiar to themselves."[84] Thus it turns out that since quantificational words are a meaningful part of language, the expressions are meaningful and yet no entities are presupposed.

> When a statement of being or nonbeing is analyzed by Russell's theory of descriptions, it ceases to contain any expression which even purports to name the alleged entity whose being is in question, so that the meaningfulness of the statement no longer can be thought to presuppose that there be such an entity.[85]

Russell's argument can also be applied to a word like 'Pegasus' which is not a descriptive phrase.

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81 ***Ibid***., p. 6.

82 ***Ibid***.

83 ***Ibid***.

84 ***Ibid***.
Using quantifiers we have non-controversial translations for the sentences in question. So the sentence "'There **is** the author of ***Waverley***'." (p. 7). is given by "'Someone (or more strictly, something) wrote ***Waverley*** and nothing else wrote ***Waverley***'." (p 7). and the sentence "'The author of ***Waverley*** is not'". (p. 7.) by "'Either each thing failed to write ***Waverley*** or two or more things wrote ***Waverley***.'" (p. 7). Of this latter case, Quine says it is false, but meaningful; and it contains no expression purporting to name the author of ***Waverley***."(p. 7).

85 ***Ibid***.

In this case we "rephrase 'Pegasus' as a description, in any way that: seems adequately to single out our idea; say 'the winged horse that was captured by Bellerophon'."[86] Using this phrase one can then analyze statements such as 'pegasus is' or 'Pegasus is not'.[87] Hence in this way it is possible to deny the existence of certain entities without thereby committing ourselves to an ontology which somehow contains the entities that we are denying exist. As Quine puts it: "We need no longer labour under the delusion that the meaningfulness of a statement containing a singular term presupposes an entity named by the term. A singular term need not name to be significant."[88] One only commits oneself to a particular entity by saying there is such an entity, and not when we deny the existence of the entity. The singular noun used in such a denial can "be expanded into a singular description, trivially or otherwise, and then analyzed out **à la** Russell."[89]

Quine emphasizes here the distinction between meaning and naming. His claim is that the tangle in Plato's beard is caused by a confusion of meaning and naming. The one disputant "confused the alleged **named object** Pegasus with the **meaning** of the word 'Pegasus', therefore

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86 ***Ibid***., p. 7.

87 Quine also considers the case where one cannot translate our word into a description:

> "If the notion of Pegasus had been so obscure or so basic a one that no pat translation into a descriptive phrase had offered itself along familiar lines, we could still have availed ourselves of the following artificial and trivial-seeming device: we could have appealed to the **ex hypothesi** unanalyzable, irreducible attribute of **being Pegasus** adopting, for its expression, the verb 'is - Pegasus', or 'pegasizes'. The noun 'Pegasus' itself could then be treated as derivative and identified after all with a description. 'the thing that is - Pegasus', 'the thing that pegasizes'." ***Ibid***., pp. 7-8.)

88 ***Ibid***., pp. 8-9.

89 ***Ibid***. p. 8.

concluding that Pegasus must be in order that the word have meaning."[90]

Conceptual Schemes and Ontology

Just how restrictive can one be with respect to ontology? The traditional problem of universals shows itself again at this point in Quine's paper with the question "whether there are such entities as attributes, relations, classes, numbers, functions."[91] A Platonist would argue that there are. For him, red houses, red sunsets, and red roses all have the attribute of redness in common. Quine sees this as part of one's metaphysics - the ontological part. The matter is a trivial question for anyone who accepts such entities for they are part and parcel of their basic conceptual scheme.

> One's ontology is basic to the conceptual scheme by which he interprets all experiences, even the most commonplace ones. Judged within some particular conceptual scheme - and how else is judgment possible? - an ontological statement goes without saying, standing in need of no separate justification at all.[92]

Thus ontology is relative to the choice of conceptual scheme. What may be obviously true from the point of view of one conceptual scheme and hence a trivial question will not be so from the point of view of another conceptual scheme.

> Judged in another conceptual scheme, an ontological statement which is axiomatic to McX's mind may, with equal immediacy and triviality, be adjudged false.[93]

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[90] ***Ibid***., p. 9.

[91] ***Ibid***.

[92] ***Ibid***., p. 10.

[93] ***Ibid***., p. 11.

Conceptual schemes bring with them their peculiar ways of interpreting the phenomena which we have before us. In some conceptual schemes words like ‘house’, ‘rose’, and ‘sunset’ may name entities. In some of those schemes and in others attributes like ‘redness’ may also be given ontological status. Here, too, we must be careful to distinguish meaning from naming. Quine says:

> McX cannot argue that predicates such as ‘red’ or ‘is-red’, which we all concur in using, must be regarded as names each of a single universal entity in order that they be meaningful at all. For we have seen that being a name of something is a much more special feature than being meaningful.[94]

The Status of Meanings

Now, however, Quine is faced with the problem of the status of meanings. What are meanings? Are they some kind of abstract entities? Quine, of course (considering his distaste for universals and the like), denies that there are such entities as meanings. However, this does not prevent him from separating expressions into meaningful ones and meaningless ones. To say an expression is meaningful is not to say that it has some abstract entity called a meaning. Quine prefers to determine the significance[95] of a linguistic expression or utterance “in terms directly of what people do in the presence of the linguistic utterance in question and other utterances similar to it.”[96] To attempt to explain the matter in terms of meanings, rather than providing an explanation merely puts off the problem until one has explained an even more mysterious entity, the meaning.

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[94] ***Ibid***.

[95] Quine prefers to use the word 'significant' rather than 'meaningful' in order to avoid the "hypostasis of meanings as entities." (***Ibid***., p. 11.)

[96] ***Ibid***., p. 11.

When people give the meaning of an utterance, Quine writes, it "is simply the uttering of a synonym, couched, ordinarily, in clearer language than the original."[97]

Synonymy plays a key role in Quine's search for a theory of meaning. We return to it later in this chapter.

Ontological Commitment:

In the latter part of 'On What There Is'(1948),[98] Quine elaborates on the notion of ontological commitment. He reiterates his claim that we only involve ourselves in ontological commitments "by our use of bound variables."[99]

> We can very easily involve ourselves in ontological commitments by saying, for example, that **there is something** (bound variable) which red houses and sunsets have in common; or that **there is something** which is a prime number larger than a million.[100]

This is the only way, however, by which we commit ourselves ontologically. The use of alleged names is not enough. Ontological commitments arise only by using names in the proper sense. What goes as a name is determined by what we countenance as values of our variables of quantification. "To be assumed as an entity is, purely and simply, to be reckoned as the value of a

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97 ***Ibid***., pp. 11-12.

98 Quine, "On What There Is", ***op. cit***.

99 ***Ibid***., p. 12.

100 ***Ibid***.

variable."[101] Quine says:

> The variables of quantification, 'something', 'nothing', 'everything', range over our whole ontology, whatever it may be; and we are convicted of a particular ontological presupposition if, and only if, the alleged presuppositum has to be reckoned among the entities over which our variables range in order to render one of our affirmations true.[102]

Quine points out that when we say "Some dogs are white", we are not thereby committing ourselves to such entities as doghood or whiteness, all that we are saying is that there are some things that are dogs and are white. He says: "...in order that this statement be true, the things over which the bound variable 'something' ranges must include some white dogs, but need not include whiteness or dogness."[103]

Quine himself, as we have seen, shows a personal preference for 'desert landscapes' and, hence, he prefers not to include whiteness and dogness among the entities named in his languages.

What are to be allowed as variables is formally determined when the rules of language are laid out. But the important question is how we decide which language is correct in allowing one set of entities to be values for its variables and not another set? How do we decide which language (with its

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101 ***Ibid***., p. 13. Quine transfers this also to notions of traditional grammar. He writes:

> "In terms of the categories of traditional grammar, this amounts roughly to saying that to be is to be in the range of reference of a pronoun. Pronouns are the basic media of reference nouns might better have been named propronouns. (***Ibid***.)

102 ***Ibid***.

103 ***Ibid***.

assigned range of values for its variables) is the correct one? How do we decide that a language that has a commitment to a great number of abstract entities (universals, classes, etc.) is inferior (or superior) to one that does not have such a commitment? Quine formulates the question as: "how are we to adjudicate among rival ontologies?"[104] His epithet, "To be is to be the value of a variable"[105] does not help, rather it serves "conversely, in testing the conformity of a given remark or doctrine to a prior ontological standard."[106] That is, we do not look to bound variables in order to determine what there is in the world, but rather in order to determine what ontology is being presupposed by the structure of the language. What that ontology is determined by some prior ontological standard.

In 'On What There Is' Quine hints at the notion he later calls **semantic ascent**. This occurs in a discussion about the reasons for debating ontological questions on a semantical level. One reason is to avoid getting tangled in Plato's beard, - that is, admitting certain entities in order to deny their existence. On a semantic level one can discuss a disagreement of ontology without involving oneself in the contradiction. This can be done by treating only the statements which the disputants affirm or deny. Another reason Quine gives for ascending to a semantical plane is "to find common ground on which to argue."[107] According to Quine, "Disagreement in ontology involves basic disagreement in conceptual schemes."[108] Since language is one of the areas where conceptual

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104 ***Ibid***., p. 15.

105 ***Ibid***.

106 ***Ibid***.

107 ***Ibid***., p. 16.

108 ***Ibid***.

schemes overlap (at least for users of the same language - other areas are things like politics, the weather), Quine feels that to argue about how to use words delays the question-begging found in ontological controversies.

The fact that we can conduct the dispute at the semantical level is not to say that the ontological issue is a linguistic issue - but just that discussion is more free at the semantical level. Quine sees a similarity between the acceptance of an ontology and the acceptance of a scientific theory.

> Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense; and the considerations which determine a reasonable construction of any part of that conceptual scheme, for example, the biological or the physical part, are not different in kind from the considerations which determine a reasonable construction of the whole. To whatever extent the adoption of any system of scientific theory may be said to be a matter of language, the same - but no more - may be said of the adoption of an ontology.[109]

The relationship between Quine's field theory of knowledge and ontology is apparent in the passage quoted above. Once one decides upon a conceptual scheme then that scheme brings with it its own particular way of viewing raw experience that is, uncategorized experience). That particular way determines the ontology for the particular language employed. The scheme is a way of categorizing experience.

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[109] ***Ibid***., pp. 16-17.

Quine employs the notion of simplicity as a criterion for choosing between alternative conceptual schemes, but, as Quine sees it, it is not a good criterion since it is capable "of presenting a double or multiple standard."[110] If one has to decide, for example, between a phenomenalistic and a physicalistic conceptual scheme, one finds that each has its own special simplicity.[111] The criterion of simplicity needs some specification if it is to provide an adequate way of deciding between the two theories.

The language of physical objects need not have preeminence. In Quine's view physical objects are on a par with convenient fictions and mythical objects. He says:

> Physical objects are **postulated entities** which round out and simplify our account of the flux of experience, just as the introduction of irrational numbers simplifies laws of arithmetic.[112]

At the end of "On What There Is" Quine admits that "the question what ontology to adopt

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[110] ***Ibid***., p. 17.

[111] A phenomenonalistic language may provide "the most economical set of concepts adequate to the **play-to-play** reporting of immediate experience." (p. 17.) Such a language would use "individual subjective events of sensation and reflection" (p. 17.) as values of the bound variables and hence as its entities. On the other hand, he says that we will find that:

> "... a physicalistic conceptual scheme purporting to talk about external objects, offers great advantages in simplifying our over-all reports by bringing together scattered sense events and treating them as perceptions of one object, we reduce the complexity of our stream of experience to a manageable conceptual simplicity." (p. 17.)
>
> So depending on what one is looking for a phenomenalistic view may be simpler or a physicalistic view may be simpler. The phenomenalistic view may be simpler. The phenomenalist one is more fundamental epistemologically, while the physicalistic is more fundamental physically.

[112] ***Ibid***., p. 18.

still stands open, and the obvious counsel is tolerance and an experimental spirit."[113] The ontological problem still remains.

Quine's field theory of knowledge in "Two Dogmas" deals with the question of adjudicating among rival ontologies. The pragmatic method developed there is in keeping with the counsel in "On What There Is."

Alternative Conceptual Schemes

Crucial to the issue of ontological commitment is the question: "How much of our science is merely contributed by language and how much is a genuine reflection of reality?"[114] Quine takes up this question in his 1950 paper entitled "Identity, Ostension and Hypothesis"[115] (a combination of a couple of lectures that he presented in 1949 entitled "On Ontologies"[116] and "Identity"[117]). He realized that this question involves us in a predicament, "for to answer the question we must talk about the world as well as about language, and to talk about the world we must already impose upon the world some conceptual scheme peculiar to our own special language."[118] However, this is not to say that we are stuck with our conceptual scheme. Quine makes his Duhemian point here by an

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[113] ***Ibid***., p. 19.

[114] W.V.0. Quine, "Identity, Ostension and Hypostasis", ***From a Logical Point of View*** p. 77.

[115] ***Ibid***., pp. 65-79. This article is also in the ***Journal of Philosophy*** XLVII, no. 22 (1950) pp. 621-633.

[116] W.V.0. Quine, "On Ontologies", lecture presented at University of Southern California in July 1949.

[117] W.V.0. Quine, "Identity", the Theodore and Grace de Laguna Lecture, given at Bryn Mawr in December 1949.

[118] Quine, "Identity..." ***op. cit***., p. 77.

appeal to Neurath's analogy of rebuilding a ship at sea.[119] Quine claims that it is possible to change our conceptual scheme "...bit by bit, plank by plank, though meanwhile there is nothing to carry us along but the evolving conceptual scheme itself."[120] It is here that we find Quine's first expression of the D-theoretic point of view. He writes:

> We can improve our conceptual scheme, our philosophy, bit by bit while continuing to depend on it for support; but we cannot detach ourselves from it and compare it objectively with an unconceptualized reality. Hence it is meaningless, I suggest, to inquire into the absolute correctness of a conceptual scheme as a mirror of reality. Our standard for appraising basic changes of conceptual scheme must be, not a realistic standard of correspondence to reality, but a pragmatic standard. Concepts are language, and the purpose of concepts and of language is efficacy in communication and in prediction. Such is the ultimate duty of language, science and philosophy, and it is in relation to that duty that a conceptual scheme has finally to be appraised.
>
> Elegance, conceptual economy, also enters as an objective. But this virtue, engaging though it is, is secondary - sometimes in one way and sometimes in another. Elegance can make the difference between a psychologically manageable conceptual scheme and one that is too unwieldy for our poor minds to cope with effectively. Where this happens, elegance is simply a means to the end of a pragmatically acceptable conceptual scheme. But elegance also enters as an end in itself - and quite properly so long as it remains secondary in another respect; namely, as long as it is appealed to only in choices where the pragmatic standard prescribes no contrary decision. Where elegance doesn't matter, we may and shall, as poets,

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[119] Otto Neurath, ***Foundations of the Social Sciences*** International Encyclopedia of Unified Science II no. 1. (Chicago, 1911). p. 47. He writes:

"Imagine sailors who, far out at sea, transform the shape of their clumsy vessel from a more circular to a more fishlike one. They make use of some drifting timber, besides the timber of the old structure, to modify the skeleton and the hull of their vessel. But they cannot put the ship in dock in order to start from scratch. During their work they stay on the old structure and deal with heavy gales and thundering waves. In transforming their ship they take care that dangerous leakages do not occur. A new ship grows out of the old one, step by step - and while they are still building, the sailors may already be thinking of a new structure, and they will not always agree with one another. The whole business will go on in a way we cannot even anticipate today."

[120] Quine, "Identity, ...", ***op. cit.***, p. 79.

pursue elegance for elegance's sake.[121]

From this passage, we can see that Quine's investigations have led him into a Duhemian position.[122] Quine's Duhemian thesis is an attempt to show that even though we always meet the world conceptualized and hence can never make the correspondence test for truth, we can discriminate among alternative conceptual schemes or alternative ontologies. This discrimination cannot be made by appeal to truth as if one could compare unconceptualized reality with the proffered conceptual schemes, but rather the discrimination is made pragmatically - on the basis of factors such as efficacy in communication and in prediction, elegance, and conceptual economy. There is no way to distinguish between theories on the basis of **truth** - that is, one theory corresponding with reality and hence being true and another not corresponding and hence being false. One cannot get at unconceptualized reality in order to conduct the test. Truth cannot be determined in this absolutist way. Quine views the matter of truth in a different way. Truth depends on the system chosen - on one's conceptual scheme. Some conceptual schemes are better than others. How one chooses between conceptual schemes or how one can say what conceptual scheme is better than another depends on what you want the conceptual scheme to do. Some of the factors involved in this evaluation are things like efficacy in communication and prediction, and the others mentioned above.

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121 ***Ibid***.

122 Quine makes a reference here to Duhem. He refers to pp. 31, 280, 347 of Duhem's book ***La Theorie Physique: son objet et sa structure*** (Paris, 1906) and also to Armand Lowinger's book, ***The Methodology of Pierre Duhem*** (New York: Columbia University Press, 1941) pp. 41, 121, 145.)

The Ontological Claims of Conceptual Schemes

Naturally, before one can compare alternative conceptual schemes, one needs to be able to sort out their ontological claims. In the early parts of the paper "Identity, Ostension, and Hypothesis," Quine discusses the manner by which one fixes the reference of our singular terms. What he says there foreshadows some of the important things that occur in ***Word and Object***. It is in this article that Quine introduces the talk of 'stages' which is so prominent in the early part of ***Word and Object***.

The talk of 'stages' develops out of a discussion of the problem of personal identity[123] and the parallel problem of Heraclitus. It is the solution to Heraclitus' problem (that "you cannot bathe in the same river twice for new waters are ever flowing in upon you."[124]) that sets the stage for Quine's later work. It lays the pattern for determining the ontological commitments of a language.

In order to solve the Heraclitean predicament a distinction is made between the river and stages of the river. "You can bathe in two river stages which are stages of the same river, and this is what constitutes bathing in the same river twice."[125] Quine's point is that the flowing river is in process and undergoing continual change and thus the river is made up of momentary parts which he calls 'river stages'. "Identification of the river bathed in once with the river bathed in again is just

123 As Quine puts it: "Considering that a complete replacement of my material substance takes place every few years, how can I be said to continue to be I for more than such a period at best?" (***Ibid.***, p. 65.)

124 ***Ibid.***, p. 65.

125 ***Ibid.***

what determines our subject matter to be a river process as opposed to a river stage."[126] Identity plays a crucial role here. He points out that "the imputation of identity is essential, here, to fixing the reference of the ostension."[127] The fact that we identify the two river stages which may be days apart in time indicates that we are not speaking of the individual stages, but rather the river of which the states are a part. Quine is aware that there are problems with this. In fact these are problems with which he takes great pains in ***Word and Object*** .

He realizes that:

> Pointing is of itself ambiguous as to the temporal spread of the indicated object. Even given that the indicated object is to be a process with considerable temporal spread, and hence a summation of momentary objects still pointing does not tell us **which** summation of momentary objects is intended, beyond the fact that the momentary object at hand is to be in the desired summation.[128]

Mere pointing, say, at a momentary part of a river does not indicate whether we are indicating the object to be a stage of the river or a stage of water. Further pointing may help to resolve this difficulty because by pointing to more and more states of the same kind, we may be able to eliminate for our observer various alternatives until eventually he has the idea of the indicated object. Another way out of the difficulty is to go beyond ostension and appeal to prior conceptualization. This is the case when one accompanies one's pointing with the saying of words like 'this river'. Here we appeal

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126 ***Ibid***.

127 ***Ibid***., p. 66.

128 ***Ibid***., p. 67.

to the listener's prior concept of river. The real problem arises, though, if our listener has no prior concept of river to which we can appeal and no amount of pointings to the alternatives gets the idea across. Quine does not consider this problem in detail here, but he does point out that:

> The concept of identity then, is seen to perform a central function in the specifying of spatio-temporally bound objects by ostension. Without identity, **n** acts of ostension merely specify up to **n** objects, each of indeterminate spatio-temporal spread. But when we affirm identity of object from ostension to ostension, we cause **n** ostensions to refer to the same large object, and so afford our listener an inductive ground from which to guess the intended reach of that object. Pure ostension plus identification conveys with the help of some induction, spatio-temporal spread.[129]

Without prior conceptualization, mere ostension is not sufficient to specify spatio-temporal objects, but one also needs identity and induction.

In the second part of the article, Quine spends time on the means by which one forms concepts of general terms such as 'red'. He points out the similarity between the learning of a general concept red and a spatio-temporal concept such as river.

> When I point in a direction where red is visible and say 'This is red' , and repeat the performance at various places over a period of time, I provide an inductive basis for gauging the intended spread of the attribute of redness.[130]

The reason for this use of identity in concept formation is a matter of convenience; - "...the entities

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129 ***Ibid***., p 68.

130 ***Ibid***., pp. 68-69.

concerned in a particular discourse are reduced from many...to one...".[131] In effect, it is a matter of economy.

As Quine puts it:

> Where what we want to say about certain broad surfaces does not concern distinctions between their parts, we simplify our discourse by making its objects as few and large as we can - taking the various broad surfaces as single objects.[132]

Quine believes that his economy measure accounts for why we contract momentary objects, such as river stages, into wholes, such as rivers, and also accounts for what Quine calls '**conceptual integration**', that is, "the integrating of particulars into a universal."[133] By means of this kind of procedure, distinctions that are not relevant to the issue being considered are removed from view. Quine propounds a maxim for this procedure which he calls the "**identification of indiscernibles**".[134] This crops up in some of his later articles. He puts the maxim as follows:

> Objects indistinguishable from one another within the terms of a given discourse should be construed as identical for that discourse. More accurately: the references to the original objects should be reconstrued for purposes of the discourse as referring to other and fewer objects, in such a way that indistinguishable originals give way each to the same new object.[135]

This very pragmatic principle is related to our discussion of ontology and the adjudication

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131 ***Ibid***., p. 70.

132 ***Ibid***.

133 ***Ibid***., p. 71.

134 ***Ibid***.

135 ***Ibid***.

of languages with rival ontologies. It would seem that one's objects or rather how one looks at the world and connects up the various parts is determined in some way by the purposes of one's discourse. One selects one's objects so that the emphasis is on the distinctions necessary to the point one wishes to get across and so that unnecessary distinctions are eliminated. There is no fundamental unit which is to be construed as an object. An object need not be continuous in space and time. Quine talks of the territory of the United States as a single concrete object,[136] even though when taken to include Alaska it is discontinuous. He talks of income groups as being just as concrete as a person or a river. As he puts it:

> ...each income group can be thought of simply as the scattered total spacio-temporal thing which is made up of the appropriate person stages, various stages of various persons. An income group is just as concrete as a river or a person, and like a person, it is a summation of person stages.[137]

In his pursuit of the concrete individual of the nominalist, Quine discovered that there is no real difference between the way one arrives at concrete individuals and the way one arrives at certain abstract entities such as universals like red. Each is found in a similar way. Thus, he says: "Up to now, therefore, the distinction between spatio-temporal integration and conceptual integration appears idle; all is spatio-temporal integration."[138]

However, there are problems with this conception of universals. It is nor that simple. Quine

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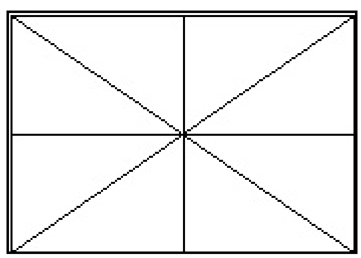
[136] ***Ibid***., p. 69.

[137] ***Ibid***., p. 72.

[138] ***Ibid***.

admits that the procedure works for general concepts like red, but it breaks down when applied to universals such as ‘triangle’ and ‘square’. Quine writes: “If square and triangle were related to the original square and triangular particulars in the way in which concrete objects are related to their momentary stages and spatial fragments, then square and triangle would turn out to be identical with each other.”[139] He points out that universals such as ‘triangle’ and‘square’ are related to their particulars in a way quite different from the way that a river is related to its river stages. The sum of a river’s stages go toward making up the river. To treat ‘triangle’ and ‘square’ as such blurs the distinction between squares and triangles. In Quine's diagram given here:



the sum of all the triangles in a spatio-temporal sense is the total region and this is identical to the sum of all the squares.

We explain ‘square’ ostensively in a different manner from the manner in which we would ostensively explain a river like ‘Cayster’. Quine writes:

[139] ***Ibid***., p. 74.

> In ostensively explaining the Cayster we point to a, b, and other stages and say each time 'This is the Cayster', identity of indicated object being understood from each occasion to the next. In ostensively explaining 'square', on the other hand, we point to various particulars and say each time 'This is square' **without** imputing identity of indicated object from one occasion to the next.[140]

This difference shows that what is identical from one ostension to the next is not an object such as a river, but rather an attribute, such as squareness, which the objects share. True to his nominalist tendencies, Quine points out that there is really no need, though to suppose that there are such entities as attributes, rather he says: "No more need be demanded, in explication of 'is square' or any other phrase, than that our listener learn when to expect us to apply it to an object and when not."[141] This is certainly in keeping with his program regarding the determination of an utterance's significance mentioned in "On What There Is"; - that is, of analyzing an utterance "in terms directly of what people do in the presence of the linguistic utterance in question and other utterances similar to it."[142]

Next, Quine summarizes what he has found to be the differences between general terms and singular terms:

> First, the ostensions which introduce a general term differ from those which introduce a singular term in that the former do not impute identity of indicated object between occasions of pointing. Second, the general term does not, or need not, purport to be a name in turn of

[140] ***Ibid***., pp. 74-75.

[141] ***Ibid***., p. 75.

[142] Quine, "On What There Is", ***op. cit***., p. 11.

a separate entity of any sort, whereas the singular term does.[143]

General terms in Quine's system do not carry an ontological commitment, rather an additional step - an appeal to a new operator 'class of' or '-ness' - is needed for the hypostasis of abstract entities. Quine claims that this is the essential difference behind the distinction between general terms like 'square' and abstract singular terms like 'squareness'. The use of the former has no ontological commitments, but the use of the latter does.

Quine's analysis here reveals his particular tendency toward nominalism. He, first, points out the similarity between learning a general concept like red and learning a spatio-temporal concept such as river. Here he seems to treat 'red' as the sum of all red things. Then, he shows how this treatment breaks down for universals such as 'triangle' and 'square'. These cannot be treated as concrete without confusion (-the confusion that could arise from this spatio-temporal treatment is demonstrated by his diagram). Quine would prefer to treat 'red' in the same fashion as the other universals 'triangle' and 'square' even though it could be treated in a manner similar to a spatio-temporal object. Because 'red' can be treated in a manner similar to spatio-temporal integration, this may account for the Platonist's reification of universals. Quine writes:

> So the theory of universals as concrete, which happened to work for red, breaks down in general. We can imagine that universals in general, as entities, insinuated themselves into our ontology in the following way. First we formed the habit of introducing spatio-temporally extended concrete things, according to the pattern considered earlier. Red entered with Cayster and the others as a concrete thing. Finally triangle, square, and other

143 Quine, "Identity, Ostension, ...", ***op. cit***., p. 75.

> universals were swept in on a faulty analogy with red and its ilk.[144]

Quine, thus, points out that we do recognize two different types of association: "that of concrete parts in a concrete whole, and that of concrete instances in an abstract universal."[145]

Quine regards ostensive introduction of terms as fundamental. This plays a crucial role in the learning of a language. We shall see later on that it is necessary to Quine's notion of stimulus meaning. In the initial stages of learning a language, ostensive introduction is important, but once one has built up a set of concepts one can employ this conceptualization in the introduction of new singular and general terms. He says that:

> ...once a fund of ostensively acquired terms is at hand there is no difficulty in explaining additional terms discursively, through paraphrase into complexes of the terms already at hand.[146]

Ostension provides the intimate connection between language and the world.

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144 ***Ibid***., p. 73.

Quine does not frown on the use of general terms. for he says: "There is every reason to rejoice that general terms are with us, whatever the cause. Clearly language would be impossible without them, and thought would come to very little." (***Ibid***., p. 77.)

He does cast his doubts on the admission of abstract entities - things named by **abstract singular** terms. These differ from **concrete general** terms (e.g. red) and **concrete singular** terms (e.g. Cäyster(for an additional operator,'class of' or '-ness' is needed for their introduction. The use of a general term e.g. 'square' does not commit us to an entity, whereas the use of an abstract singular term does, e.g. 'squareness'.

145 ***Ibid***., p. 74.

146 ***Ibid***., p. 78.

Singular Terms and Quine's Preferences

Quine has some personal preferences with respect to the matter of singular terms. These show through in the preceding analysis, but Quine makes them more explicit in his 1950 paper "Semantics and Abstract Objects".[147] He begins that paper by deploring two tendencies that he finds in the philosophy of language. The first is the "tendency to confuse meaning with reference."[148] The second is the "tendency to widen excessively the category of singular terms."[149] We have already discussed Quine's response to the former at some length. He followed Frege in distinguishing meaning from reference by means of the Morning Star - Evening Star example.

The two tendencies tend to run together since Quine prefers to reserve naming as the function of singular terms. Quine (exhibiting his nominalist tendencies) prefers to keep the class of names narrow (recall his love for desert landscapes). He does not want to include either statements or general terms in the class of names. For example, the tendency to include statements in the class of names, he believes, results from the confusion that some have made in identifying the meaning of a statement with its reference. He says:

> Statements have frequently been treated as names of propositions, these latter being construed as entities of a sort better describable as **meanings** of statements.[150]

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[147] Quine, "Semantics..." see footnote 3.

[148] ***Ibid***., p. 90.

[149] ***Ibid***.

[150] ***Ibid***.

Quine, as we have already noted, sees no need for mysterious things such as meanings or propositions. Frege, for example, considered both general terms and statements to serve as names: the general terms named their extension ("the class of all things of which the term is true"[151]) and a statement named "its truth value"[152].

Quine believes that the need to have meant entities or propositions probably derives from the failure to keep meaning and naming distinct. He writes:

> Once the theory of meaning is sharply separated from the theory of reference, it is a short step to recognizing as the business of the theory of meaning simply the synonymy of expressions, the meaningfulness of expressions, and the analyticity or entailment of statements; meanings themselves, as obscure intermediary entities, may well be abandoned.[153]

Quine is clearly aware that his constraints with respect to singular terms reflect his particular preferences and as such he seems to acknowledge the fact that theories built upon other sets of preferences are at least possible. He writes:

> I prefer to consider that naming is the function of singular terms in the original sense, excluding general terms and statements.[154]

In his theory of reference Quine gives general terms and statements referential functions that are

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[151] ***Ibid***.

[152] ***Ibid***.

[153] ***Ibid***., p. 91.

[154] ***Ibid***., p. 92.

quite different from naming and meaning.

> General terms and statements have referential functions of their own, but of a different type than meaning a general term is **true of** this and that object and **false of** this and that object, and a statement is true or false.[155]

He puts it another way:

> A general term **has** its extension, viz. the class of all things of which it is true, and a statement **has** its truth value; but there is no need to treat the general term as a name of its extension, nor the statement as a name of its truth value.[156]

In order to clarify the concept of naming, Quine returns to his principle "To be is to be the value of a variable". The objects which can be named in a given language which has quantification are "... just the things which must fall within the range of our variables of quantification in order that the statements we affirm come out true."[157] Quine here elaborates on his objections to Frege's treatment of general terms and statements. He objects to permitting general terms to be substituted for quantified class variables and statements to be substituted for quantified truth value variables. The reason that Quine gives for his objections, as we have seen before, is "that it leads to a false accounting of the ontological presuppositions of most of our discourse."[158] He is most concerned that one keep track of the ontological presuppositions of one's theories. He writes:

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155 ***Ibid***.

156 ***Ibid***., pp. 92-93.

157 ***Ibid***., p. 93.

158 ***Ibid***.

> The important thing is to understand our instrument; to keep tab on the diverse presuppositions of diverse portions of our theory, and reduce them where we can. It is thus that we shall best be prepared to discover, eventually, the over-all dispensability of some assumption that always ranked as **ad hoc** and unintuitive.[159]

Quine's reason for minimizing the ontological presuppositions of one's theory is the pragmatic one of keeping our tools in good working order. The assumption is that an understanding of the ontological presuppositions of one's instruments will help to avoid errors. Thus he deplores widening the category of singular terms beyond what is really necessary. He presents his reasons for this in greater detail in a paper entitled 'Logic and the Reification of Universals'[160].

That paper is primarily devoted to the use of quantification as a means of determining the ontological commitments of a given theory. By means of quantification one can determine what objects a given theory depends on and what objects could be dispensed with. He writes here:

> In general, entities of a given sort are assumed by a theory if and only if some of them must be counted among the values of the variables in order that the statements affirmed in the theory be true.[161]

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159 ***Ibid***., p. 96.

160 W.V.0. Quine, "Logic and the Reification of Universals" ***From a Logical Point of View***, pp. 102-109. This paper derives mainly from work that he did in 1947, primarily in a paper presented to the Association of Symbolic Logic in February 1947 called "On the Problem of Universals". Quine tells us that part of that paper was published in the ***Journal of Symbolic Logic*** in 1947 in the article "On Universals" (***op. cit***.) (see p. 170 in ***From a Logical Point of View***). "Logic and the Reification of Universals" also makes use of parts of "Semantics and Abstract Objects" (***op. cit***.) and "Designation and Existence" (***op. cit***.). So we can see that the matters covered in this article have occupied Quine for many years (1939-1950) and therefore this article is very important to the understanding of Quine's philosophical development during this period.

161 Quine, "Logic ..." ***Ibid***., p. 103. (Quine's italics)

He stresses again that he is not saying that existence or being is dependent upon language. "What is under consideration is not the ontological state of affairs, but the ontological commitments of a discourse."[162] Although one's use of language does not determine what there is in the world, Quine tells us that it does determine "what one says there is."[163] He is talking about the ontological commitments of discourse. A person may or may not share the ontological commitments of his discourse. As Quine puts it: "The parent who tells the Cinderella story is no more committed to admitting a fairy godmother and a pumpkin coach into his ontology than to admitting the story as true."[164] He points to the situation where on first glance a theory has a commitment to the existence of certain objects but further development may show these to be just convenient fictions.[165]

The reason why Quine believes the theory of quantification to be a good standard of ontological commitments is that: "Every statement containing a variable can be translated, by known rules, into a statement in which the variable has only the quantificational use."[166] He grants that there are other theories that could equally well be taken as the criterion of ontological commitment.

> It is equally true that any statement containing variables can be translated, by other rules, into a statement in which variables are used solely for class abstraction; and, by still other rules,

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[162] ***Ibid***.

[163] ***Ibid***.

[164] ***Ibid***.

[165] For example, the case that we discussed earlier that of extending quantification definitionally. See p. 66 of ***The Ways of Paradox*** in "A Logistical Approach to the Ontological Problem".

[166] ***Ibid***., p. 104. see W.V.0. Quine, "New Foundations for Mathematical Logic" ***From a Logical Point of View*** p. 85 and following.

into a statement in which variables are used solely for functional abstraction.[167]

He takes account, for example, of the combinatory method of Schönfinkel and Curry which "gets rid of variables altogether by recourse to a system of constants, called combinators, which express certain logical functions."[168] The ontological commitments of this kind of discourse can be determined by a systematic translation from combinatory discourse to quantificational discourse. Also Quine writes that:

> ...there is no difficulty in devising an equivalent criterion of ontological commitment for combinatory discourse. The entities presupposed by statements which use combinators turn out, under such reasoning, to be just the entities that must be reckoned as arguments or values of functions in order that the statements in question be true.[169]

Quine has constructed his theory of quantification in such a way as to draw out the contrast between those terms which name and hence have ontological commitments and those which do not. He regards the valid forms of the logic of quantification "simply as schemata or diagrams embodying the form of each of various true statements."[170] Thus in an expression such as:

(1.)$[(x)(Fx \supset Gx) \cdot (\exists x)Fx] \supset (\exists x)Gx$

167 ***Ibid.***, p. 104. see A. Church, "A Set of Postulates for the Foundation of Logic" ***Annals of Mathematics*** vol. 33 (1932), pp. 346-366, vol. 34 (1933), pp. 839-864.

168 Quine, "Logic ..." ***Ibid.***, p. 104.

169 ***Ibid.***

170 ***Ibid.***, p. 108.

Quine says "...there is no need to view the 'F' and 'G' of (1.) as variables taking classes or anything else as values."[171] 'F' and 'G' are blanks in the sentence diagram to be filled in by predicates; they are not bindable variables and hence do not have any ontological commitments to classes or anything else.

Similarly in the logic of truth functions, 'p' and 'q' are used to take the place of the component statements in principles such as : '$[(p \supset q) \cdot \sim q] \supset \sim p$'. Instead of regarding 'p' and 'q' as variables taking some sort of entities (for example, propositions which are named by statements) as values, since they are not used as bindable variables, we can regard them as schematic letters. Thus, "we can view '$[(p \supset q) \cdot \sim q] \supset \sim p$', like (1.) not as a sentence but as a schema or diagram such that all actual statements of the depicted form are true."[172] The 'p' and 'q' are also to be regarded as blanks in the sentence diagram.

> The schematic letters 'p', 'q', etc. stand in schemata to take the place of component statements, just as the schemata to take the place of predicates; and there is nothing in the logic of truth functions or quantification to cause us to view statements or predicates as names of any entities, or to cause us to view these schematic letters as variables taking any such entities as values.[173]

Quine's view of quantification requires a classification of expressions. An expression like $x + 3 > 7$ is an **open sentence**. It contains a free 'x' and is "capable of occurring within a context

[171] ***Ibid***.

[172] ***Ibid***., p. 109.

[173] ***Ibid***.

of quantification to form part of a statement,"[174] otherwise it would be closed. An expression like (x)(Fx ⊃ p) is a schema, not a sentence. This is because 'F' and 'p' are schematic letters and hence are not bindable variables. Such an expression, then, "cannot be imbedded within quantification to form part of a statement."[175] Another kind of expression employs Greek letters, for example, $(\exists\, \alpha)(\varphi \vee \alpha)$. This kind of expression "stands as a name **of** a sentence, or comes to do so as soon as we specify a particular choice of expressions for the Greek letters to refer to."[176] Here the Greek letters operate as variables only they are "variables within a portion of language specifically designed for talking **about** language."[177] Just as 'x' in the expression $x+3 > 7$ can take numbers as values "and thus stands in place of names of numbers; now correspondingly the Greek letters are variables which take sentences or other expressions as values, and thus stand in place of names (for example, quotations) of such expressions."[178] Thus the Greek letters are bindable variables and are not like our schematic letters. A schema does not name anything, but merely manifests "a form which various sentences manifest."[179]

Quine presents his reasons for making these distinctions in the use of letters as follows:

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174 ***Ibid***.

175 ***Ibid***.

176 ***Ibid***., p. 111.

177 ***Ibid***.

178 ***Ibid***.

179 ***Ibid***.

> The significance of preserving a schematic status for 'p', 'q', etc. and 'F', 'G', etc., rather than treating those letters as bindable variables, is that we are thereby (a) forbidden to subject those letters to quantification, and (b) spared viewing statements and predicates as names of anything.[180]

Now a critic of Quine might accuse him of making such a distinction merely because he does not wish to admit classes and truth values into his ontology. Quine, on the other hand, now grants that there could be good reasons for accepting classes into one's ontology. (If one did so, then one would use them to replace bindable variables such as 'x' and 'y'.) However, he does not want to treat "statements and predicates as names of such or any entities, and thus identifying the 'p', 'q' etc. of truth-function theory and the 'F', 'G', etc. of quantification theory with bindable variables."[181] One of Quine's reasons for making the distinction "is that to construe 'Fx' as affirming membership of x in a class can, in many theories of classes, lead to a technical impasse."[182] He is referring to some of the problems that provoked the use of a theory of types.[183] To blur the distinction between bindable variables and schematic letters tends to obscure some features of classes.

> For there are theories of classes in which not every expressible condition on x determines a class, and theories in which not every object is eligible for membership in classes. In such a theory 'Fx' can represent any condition whatever on any object x, whereas '$x \in y$' cannot.[184]

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180 ***Ibid***., p. 112.

181 ***Ibid***., p. 113.

182 ***Ibid***.

183 See his "New Foundations of Mathematical Logic" ***op. cit***.

184 Quine, "Logic...", ***op. cit***., p. 113.
In this passage, Quine is referring to conditions that would generate the class of all classes that are not members of themselves, and such a class (i.e. purported class since there can be no such class) generates Russell's paradox which necessitates something like a theory of types. Similar problems

Beyond this claim, though, our critic is right, for Quine says that "the main disadvantage of assimilating schematic letters to bound variables is that it leads to a false accounting of the ontological commitments of most of our discourse."[185] Quine clearly does not want to commit himself to abstract entities such as dogkind or the class of white things. Again, though, we should point out that this is a matter of preference on Quine's part.

He does permit one to have class variables. To do this one can use a more explicit form than $(\exists x)(Fx \cdot Gx)$, such as $(\exists x)(x \in y \cdot x \in z)$ which indicates that we are using classes as values of variables or else instead of using 'y' and 'z', we could use a different set of letters for classes. Now 'y' and 'z' can be replaced by singular terms which name our abstract entities, for example,

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arise with impredicable properties - that is, any property that cannot be truly predicated of itself. Irving Copi shows us how we can generate a contradiction using bindable predicate variables. "The contradiction can be derived more clearly by symbolizing the property of being impredicable as 'I', and defining it formally as

$$IF = \text{df.} \sim FF,$$

which definition has the following general proposition as an immediate logical consequence:

$$(F)\ (IF \equiv \sim FF).$$

From the latter, by the principle of Universal Instantiation, we can instantiate with respect to 'I' itself to obtain

$$II \equiv \sim II$$

which is an explicit contradiction." (I. Copi, ***Symbolic Logic***, (New York, 1954) p. 162.)

185 ***Ibid***.

$(\exists x)\ (x \in \text{dogkind} \ . \ x \in \text{class of white things})$

Thus the commitment to abstract entities such as dogkind and the class of white things is made explicit. Our first expression $(\exists\, x)(Fx \cdot Gx)$, where 'Fx' is 'x is a dog' and 'Gx' is 'x is white', is neutral regarding class existence, but our second expression $(\exists x)(\ x \ni y \ . \ x \ni z)$ fits that language which takes classes as values of variables.

Quine, in contrast to his usual nominalist tendencies allows that there are situations where one needs to permit classes to be values of variables of quantification. "One such occasion arises when we define ancestor in terms of parent, by Frege's method: **x** is ancestor of **y** if **x** belongs to **every class** which contains **y** and all parents of its own members."[186] Quine, thus, admits there are occasions where singular terms are permitted to name classes. However, he does not permit general terms or predicates to name. Thus if we are going to hold that classes are to be admitted into our ontology they must be admitted as values for our singular terms and not for predicates. This does not mean that there are not often certain classes which are connected with our predicates. It means that the connection is not one of naming. He tells us that:

> Occasions arise for speaking of the **extension** of a general term or predicate - the class of all things of which the predicate is true.[187]

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[186] ***Ibid***., p. 115.

[187] ***Ibid***.

In spite of the special use of abstract entities such as classes within some kinds of theories, one must be careful in one's analysis of theory or discourse to get the ontological commitments straight. Quine's construal of quantification theory is designed to avoid ambiguities in the determination of the ontological commitments of a particular theory and for this reason his method of analysis is highly recommended in deciding ontological disputes.

It is clear, nevertheless, that Quine is pushing for a reduced ontology. In the final paragraphs of this section, he reasserts that most logical tasks can be performed by means of quantification theory and identity theory without any appeal to classes and other kinds of abstract entities and he, therefore, considers it "a defect in an all-purpose formulation of the theory of reference if it represents us as referring to abstract entities from the very beginning rather than only where there is a real purpose in such reference."[188] One can ask why is there this constant drive for the reduction of entities, for a theory which uses abstract entities might be less clumsy than one that eliminates them altogether. Quine does not deal with this question here. The best reason that he gives for his program is that it is to enable us to understand even the most diverse parts of our theory and in this way "we shall best be prepared to discover, eventually, the over-all dispensability of some assumption that has always rankled as **ad hoc** and unintuitive."[189]

Synonymy

Quine has been grappling with the theory of meaning. He has been careful to show that

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[188] ***Ibid***., p. 116.

[189] ***Ibid***., p. 117.

naming involves something more than mere significance. A term may be meaningful or significant and still not be a name. Naming involves an additional step. Ontological commitments come with a claim that **there is** a particular entity and not merely by virtue of the meaningful use of a term. A reference theory of meaning is inadequate. Reference certainly involves the use of meaningful discourse, but it goes beyond that use.

In "On What There Is", Quine considered synonymy as the key to the theory of meaning. When people give the meaning of an utterance, it "is simply the uttering of a synonym, couched ordinarily, in clearer language than the original."[190] We have seen that he regards the theory of reference as involving the concepts of naming, truth and denotation. In contrast, he considers "the main concepts in the theory of meaning apart from meaning itself are **synonymy** (or sameness of meaning), **significance** (or possession of meaning), **analyticity** (or truth by virtue of meaning)."[191] As we shall see, all of these concepts are intimately related.

Quine realizes, though, that this notion of synonymy is also vague and in need of clarification and elaboration. In his 1943 paper "Notes on Existence and Necessity", he had already indicated the vagueness of the notions of meaning and synonymy. In that paper he had drawn the distinction between meaning and reference and had pointed out that the question of synonymy or sameness of meaning was not a matter of reference. He wrote:

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190 Quine, "On What ..." ***op. cit***., pp. 11-12.

191 Quine, "Semantics ..." ***op. cit***., p. 91.

> To say that two names designate the same object is not to say that they are **synonymous**, that is, that they have the same meaning. To determine the synonymity (sic) of two names or other expressions it should be sufficient to understand the expressions; but to determine that two names designate the same object, it is commonly necessary to investigate the world.[192]

Synonymy is clearly a matter of meaning. In that paper Quine was also perplexed over the question of the status of meanings.

> Just what the meaning of an expression is what kind of object - is not yet clear; but it is clear that, given a notion of meaning, we can explain the notion of **synonymity** (sic) easily as the relation between expressions that have the same meaning. Conversely also, given the relation of synonymity (sic) it would be easy to derive the notion of meaning in the following way: the meaning of an expression is the class of all expressions synonymous with it. No doubt this second direction of construction is the more promising one. The relation of synonymity (sic), in turn, calls for a definition of a criterion in psychological and linguistic terms. Such a definition, which up to the present has perhaps never even been sketched, would be a fundamental contribution at once to philology and philosophy.[193]

To point out why this would be such a fundamental contribution Quine indicated the importance of the notion of synonymy for philosophical notions other than that of meaning. For example, we rely on the notion of synonymy whenever we use indirect quotations. "In indirect quotations we do not insist on a literal repetition of the words of the person quoted, but we insist on a **synonymous** sentence; we require reproduction of the **meaning**."[194] Also, the notion of synonymy is presupposed in the notion of analyticity. He writes: "It is usual to describe an analytic statement as a statement that is true by virtue of the **meanings** of the words; or as a statement that follows

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192 Quine, 'Notes ...', ***op. cit***., p. 119. Quine uses 'synonymity' interchangeably with 'synonymy'.

193 ***Ibid***., p. 120.

194 ***Ibid***.

logically from the meanings of the words."[195] This is an analysis that he follows up in more detail in the "Two Dogmas" article, but the gist of the argument is found in "Notes".

> Given the notion of synonymity (sic), given also the general notion of truth, and given finally the notion of logical form (perhaps by an enumeration of the logical vocabulary), we can define an analytic statement as any statement which, by putting synonyms for synonyms, is convertible into an instance of a logical form all of whose instances are true.[196]

The notion of necessity also relies indirectly on the notion of synonymy since in one sense it can be defined in terms of analyticity. Quine writes: "the result of applying 'necessarily' to a statement is true if, and only if, the original statement is analytic."[197] Also since the notions of 'possibly' and 'it is impossible that...' are definable on the basis of 'necessarily'[198] then these depend indirectly on the notion of synonymy. Since indirect quotation, analyticity, and the modal notions of necessity, impossibility, and possibility are all dependent on synonymy, then clarification of synonymy is fundamentally important.

Quine develops further suspicions about the notion of synonymy in his 1947 paper "The Problem of Interpreting Modal Logic."[199] Since, as we saw above, the notion of synonymy is closely

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195 ***Ibid***.

196 ***Ibid***.

197 ***Ibid***., p. 121.

198 'possibly' =df. 'not necessarily not'.
'it is impossible that'= df. 'necessarily not'.

199 W.V.0. Quine, "The Problem of Interpreting Modal Logic", ***Journal of Symbolic Logic*** XII, no. 2 (June, 1947) pp. 43-48.

related to the notion of analyticity, so these suspicions also lead to suspicions about the notion of analyticity. He begins in this paper by claiming that every logical truth is "**deducible by the logic of truth-functions and quantification from true statements containing only logical signs**,"[200] and also that "the class of analytic statements is broader than that of logical truths, for it contains in addition such statements as 'No bachelor is married.'"[201] (It would seem that in this paper Quine believes that there is a definitive class of statements which could be characterized as analytic.) In this paper he defines 'analyticity' using the notion of synonymy and the notion of logical truth (above):

> a statement is **analytic** if by putting synonyms for synonyms (e.g. 'man not married' for 'bachelor') it can be turned into a logical truth.[202]

Here he is referring to the replacement of 'bachelor' in a sentence like 'No bachelor is married' by 'man not married' to get the logical truth 'No man not married is married' (or better in 'A bachelor is a man not married' to get 'A man not married is a man not married.')

In this article he also gives another definition of synonymy in terms of analyticity:

> Statements are synonymous if the biconditional ('if and only if') which joins them is analytic: names are synonymous if the statement of identity which joins them is analytic; and

200 ***Ibid***., p. 43.

201 ***Ibid***., p. 44.

202 ***Ibid***.

> predicates are synonymous if, when they are applied to like variables and then combined into a universally quantified biconditional, the result is analytic.[203]

He will make use of this definition later on. Of course, he realizes that a different notion of synonymy is needed if one is to avoid the circle of defining synonymy in terms of analyticity which is itself defined in terms of synonymy. Yet he is unable to give a satisfactory definition of the tetradic relation of synonymy ("the expression x is synonymous with the expression y for person z at time t").[204] Nevertheless he thought that a satisfactory definition was forth-coming. He wrote: "So long, however, as we persist in speaking of expressions as alike or unlike in meaning..., we must suppose that there is an eventually formulable criterion of synonymy in some reasonable sense of the term."[205]

In "Semantics and Abstract Objects"[206] (1950), Quine tackles the question of the kind of things meanings are. As we have seen, he blames the need to have meant entities (propositions and what have you) on the "failure to appreciate that meaning and naming are distinct."[207] He writes here and again in "Two Dogmas" that:

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203 ***Ibid***.

204 ***Ibid***.

205 ***Ibid***. In spite of the fact that analyticity has not yet been firmly grounded, Quine still believes that the notion of analyticity is clearer than the notions of modal logic and he feels justified in explaining the notions of modal logic in terms of analyticity. He attempts to do this in the next part of his paper.

206 Quine, "Semantics ...", ***op. cit***.

207 ***Ibid***., p. 91.

> Once the theory of meaning is sharply separated from the theory of reference, it is a short step to recognizing as the business of the theory of meaning simply the synonymy of expressions, the meaningfulness of expressions and the analyticity or entailment of statements; meanings themselves, as obscure intermediary entities, may well be abandoned.[208]

He again summarizes the relationships found among the various notions of the theory of meaning.

> Statements are synonymous, in a broad sense, if their biconditional is analytic, or in other words if they entail each other. Singular terms are synonymous if their identity is analytic. Predicates are synonymous if, when they are applied to variables, their universally quantified biconditional is analytic. An expression is meaningful if synonymous with itself. A statement is analytic if synonymous in our broad sense, with some arbitrarily chosen specimen - say '(x)(x = x)'.[209]

Another notion, the empiricist's notion of confirmation, can also be linked to the notions of synonymy and analyticity. Quine wrote:

> As an empiricist I consider that the cognitive synonymy of statements should consist in sameness of the empirical conditions of their confirmation. A statement is analytic when its operational condition of verification is, so to speak, the null condition.[210]

The problem is that the theory of confirmation is far from clear. The danger is in explaining confirmation in terms of synonymy and analyticity. Quine warns: "I think philosophers are tending

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[208] ***Ibid***. , p. 91 and in "Two Dogmas ..." ***op. cit***. p. 22

[209] ***Ibid***., ("Semantics ...") p. 92.

[210] ***Ibid***.

to be insufficiently chary of the circularity involved in resting their eventual account of confirmation upon such concepts as synonymy and analyticity."[211]

Two Dogmas of Empiricism:

In "Two Dogmas of Empiricism", Quine brings together the issues that were of concern in his earlier papers and begins to develop a systematic approach that encompasses them all. The paper starts as an attack on the Logical Empiricist's dogma which bifurcates statements into those that are analytic and those that are synthetic and then it attacks the dogma of reductionism. The paper ends in a metaphorical presentation of his field theory of knowledge - a theory which touches on the problems of meaning and reference, ontological commitment, and theory construction. Reference and ontology are connected through the process of naming. Meaning and theory construction are tied through the creation of conceptual frameworks. Meaning and reference tie up through intimate logical and linguistic connections.

Quine begins with an analysis of the analytic-synthetic dogma which, as we have seen, brings in all the issues of meaning. This is because analytic truths are purported to be true by virtue of their meaning independent of any matter of fact, whereas synthetic truths are true contingent upon the posture of the world. Quine searches for a clarification of the distinction.

After briefly noting the history of the dogma with respect to Hume, Leibniz, and Kant, he dismisses

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[211] ***Ibid***.

the explanation that analytic statements are “statements whose denials are self-contradictory.”[212] His complaint is that the notion of self-contradiction is as vague as the notion of analyticity and thus any attempt to explain analyticity in terms of self-contradiction merely puts off the explanation.

Next, he rejects Kant’s conception of an analytic statement “as one that attributes to its subject no more than is already conceptually contained in the subject.”[213] He has two reasons for this rejection. The first is that it narrowly applies only to statements of subject-predicate form. The second is that the notion of containment is used metaphorically and hence in need of elaboration.

Quine spends considerably more time on the definition of analyticity in terms of meaning, that is, “a statement is analytic when it is true by virtue of meanings and independently of fact.”[214] This view of analyticity presupposes a certain conception of meaning. Here he introduces a point that he made in his early writing that meaning is distinct from naming. For example, where the expressions ‘Evening Star’ and ‘Morning Star’ name the same things, - they have the same reference-, these terms do not have the same meaning. This distinction holds for both concrete and abstract singular terms.[215] There is a parallel situation with general terms and predicates. One must

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212 Quine, "Two Dogmas ..." ***op. cit***., p. 20.

213 ***Ibid***., pp. 20-21.

214 ***Ibid***., p. 21.

215 Quine's examples of concrete singular terms are 'Evening Star', 'Morning Star', 'Scott', and 'the author of ***Waverley***'. His examples of abstract singular terms are '9' and 'the number of planets'. In the latter case the two expressions name the same abstract entity but, of course, differ in meaning. In fact, empirical observations of an astronomical nature have to be made in order to determine that they do name the same entity.

distinguish between the meaning of a general term and its extension.

> Whereas a singular term purports to name an entity, abstract or concrete, a general term does not; but a general term is **true of** an entity, or of each, or of none. The class of all entities of which a general term is true is called the **extension** of the term.[216]

As we have noted in our discussion of "On What There Is", Quine believes that in sharply distinguishing theory of reference from theory of meaning one removed the need to have meanings or meant entities. One recognized "as the primary business of the theory of meaning simply the synonymy of linguistic forms and the analyticity of statements."[217] It follows, then, that to define analyticity in terms of meaning results in a circle.

Quine, next, selects statements that are commonly claimed to be analytic and subjects them to analysis. He sorts them into two classes: those that are logically true and those which "can be turned into a logical truth by putting synonyms for synonyms."[218] A logically true statement, according to Quine, "is a statement which is true and remains true under all reinterpretations of its components other than the logical particles."[219] An example of a logical truth is the statement "No

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216 ***Ibid***. Quine's examples of general terms are 'creature with a heart' and 'creature with kidneys'. It is scientifically known that they have the same extension, but these expressions, of course, differ in meaning.

217 ***Ibid***., p. 22.

218 ***Ibid***., p. 23.

219 ***Ibid***., pp. 22-23.

unmarried man is married."[220] An example of the second class of analytic truths is the statement "No bachelor is married."[221] This statement can be turned into a logical truth by substituting for 'bachelor' its synonym 'unmarried man'. However, since the notion of synonym is also in need of clarification, the general notion of analyticity is not clarified by this analysis.

Carnap sometimes explained analyticity in terms of state-descriptions (-"any exhaustive assignment of truth values to the atomic, or noncompound, statements of the language."[222]) Quine passes quickly over this attempt because he thinks it is only a reconstruction of the notion of logical truth[223] and not analyticity. On Carnap's view, a statement is analytic "when it comes out true under every state description,"[224] i.e., true for all possible worlds. Quine complains that:

> ...The criterion of analyticity in terms of state-descriptions serves only for languages devoid of extra-logical synonym-pairs, such as 'bachelor' and 'unmarried man' - synonym-pairs of the type which give to the 'second class' of analytic statements.[225]

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220 ***Ibid***., p. 23.

221 ***Ibid***.

222 ***Ibid***.

223 On Carnap's view - "a statement is then explained as analytic when it comes out true under every state description." (***Ibid***., p. 23.) "A state-description is any exhaustive assignment of truth values to the atomic or noncompound, statements of the language." (***Ibid***.) The complex statements of a language are logically built up from the atomic statements and so their truth-value can be fixed using logical techniques.

224 ***Ibid***.

225 ***Ibid***.

Quine's example shows why this is so. He says:

"But note that this version of analyticity serves its purpose only if the atomic statements of

Thus, according to Quine, Carnap's approach only clarifies our first class of analytic statements and does not account for the second class.

Next Quine treats the attempt to explain the second class of analytic truths using the notion of definition instead of the notion of synonymy. On this account, the second kind of analytic truths reduce to logical truths **by definition**. The reduction takes place because 'bachelor' is defined as 'unmarried man'. The problem with this account is that this, too, depends on the notion of synonymy. This becomes clear when one asks how we arrive at the definition employed. It turns out that the definition is the report of an observed synonymy of terms. People use the words synonymously. Quine writes that: "the notion of synonymy presupposed here has still to be clarified, presumably in terms relating to linguistic behavior."[226] One thing is clear; the interconnections between two synonymous terms are ordinarily based upon usage, even though it is not clear just what the necessary and sufficient conditions are for one term to be said to be synonymous to another.

Quine considers two other notions of definition, that of explication and that of convention. Both of these depend on the notion of synonymy even though they do not depend on preexisting synonymy.

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the language are, unlike 'John is a bachelor' and 'John is married', mutually independent. Otherwise there would be a state-description which assigned truth to 'John is a bachelor' and to 'John is married', and consequently 'No bachelors are married' would turn out synthetic rather than analytic under the proposed criterion." (***Ibid***., p. 23.)

[226] ***Ibid***., p. 24.

> In explication the purpose is not merely to paraphrase the definiendum into an outright synonym, but actually to improve upon the definiendum by refining or supplementing its meaning. But even explication, though not merely reporting a preexisting synonymy between definiendum and definiens, does rest nevertheless on **other** preëxisting synonymies.[227]

Quine's point is that if a definition is to be suitable for explication while the terms themselves need not be antecedently synonymous, the whole context in which the one term, the definiendum, is found must be synonymous with the corresponding context of the other term, the definiens. It may even be the case that the two terms are not synonymous in all contexts, but they must be synonymous in the contexts in question if they are to be used interchangeably in those contexts. In the case of explicit conventions where for the purposes of abbreviation a new expression is introduced to replace another expression, "the definiendum becomes synonymous with the definiens simply because it has been created expressly for the purpose of being synonymous with the definiens."[228] Thus since the notion of definition depends on the notion of synonymy. Quine decides to concentrate on the notion of synonymy to get at analyticity.

The first suggestion that he examines is that "the synonymy of two linguistic forms consists simply in their interchangeability in all contexts without change of truth value."[229] However, if one considers contexts with phrases like 'bachelor of arts' then, even the synonyms 'bachelor' and 'unmarried man' are not interchangeable in all contexts. This kind of counter-example can be put

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[227] ***Ibid***., p. 25.

[228] ***Ibid***., p. 26.

[229] ***Ibid***., p. 27.

aside by considering the phrase ‘bachelor of arts’ as a different word than ‘bachelor’ and stipulating that synonymy is not “to apply to fragmentary occurrences inside of a word.”[230] (Quine grants that this notion of ‘wordhood’ is vague.) He wants to determine whether interchangeability in all contexts - except within words - without change of truth value is a sufficient condition for the cognitive synonymy[231] of two linguistic expressions.

As we have seen from Quine’s earlier work, one could define cognitive synonymy in terms of analyticity. He tells us here that:

> ... to say that ‘bachelor’ and ‘unmarried men’ are cognitively synonymous is to say no more nor less than that the statement:
>
> (3) All and only bachelors are married men is analytic.[232]

But this, of course, does not help us with our problem of getting at analyticity through synonymy.

Quine gives an example to show that interchangeability **salva veritate**, i.e., without change of truth value (except within words), is a sufficient condition for cognitive synonymy.

The statement:

(4) Necessarily all and only bachelors are bachelors

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230 ***Ibid***., p. 28.

231 ***Ibid***., p. 29.

232 ***Ibid***., pp. 28-29.

> is evidently true, even supposing 'necessarily' so narrowly construed as to be truly applicable only to analytic statements. Then, if 'bachelor' and 'unmarried man' are interchangeable **salva veritate**, the result:
>
> (5) Necessarily all and only bachelors are unmarried men.
>
> of putting 'unmarried man' for an occurrence of 'bachelor' in (4) must, like (4), be true. But to say that (5) is true is to say that (3) is analytic, and hence that 'bachelor' and 'unmarried man' are cognitively synonymous.[233]

The problem, though, has to do with the richness of the language in question - for the language needs to be rich enough to contain the intensional adverb 'necessarily'. But, as you can see, by the example this adverb is designed "to yield truth when and only when applied to an analytic statement."[234] Hence such an adverb can only make sense after we have a sufficient notion of analytic. (Something that Quine had noticed in his earlier work).

Quine's point is that in a language that is richer than an extensional language, one can get a sufficient condition for cognitive synonymy. In a language which contains the intensional adverb 'necessarily' or a similar particle, the condition of interchangeability without change of truth value is sufficient. Such a language, to be intelligible, though, presupposes a notion of analyticity, for necessity is explained in terms of analyticity (see Quine's "The Problem of Interpreting Modal Logic").

Quine makes it clear that he is not relying on the extensional agreement of terms like

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[233] ***Ibid***., p. 29.

[234] ***Ibid***., pp. 29-30.

‘bachelor’ and ‘unmarried man’ to determine cognitive synonymy, for extensional agreement falls short of explaining cognitive synonymy since extensional agreement is not a question of meaning. Expressions like ‘creature with a heart’ and ‘creature with kidneys’, while they agree extensionally, they are not synonymous; - their meanings are quite different. Thus interchangeability without change of truth value is not sufficient to determine cognitive synonymy for an extensional language.

Returning to his example he says:

> That ‘bachelor’ and ‘unmarried man’ are interchangeable **salva veritate** in an extensional language assures us of no more than that (3) is true.[235]

That (3) is true could be an accidental feature of language (as in case of ‘creature with a heart’ and ‘creature with kidneys’), but we require something stronger - namely a condition that will make (3) analytically true.

As we saw in Quine's 1943 paper (“Notes”) and his 1947 paper (“The Problem of Interpreting Modal Logic”), other related notions also depend on the notion of analyticity. As he puts it in “Two Dogmas”:

> We have see that cognitive synonymy of ‘bachelor’ and ‘unmarried man’ can be explained as analyticity of (3). The same explanation works for any pair of one-place predicates, of course, and it can be extended in obvious fashion to many-place predicates. Other syntactical categories can also be accommodated in fairly parallel fashion. Singular terms may be said to be cognitively synonymous when the statement of identity formed by putting ‘=’ between them is analytic. Statements may be said simply to be cognitively synonymous when their

[235] ***Ibid***., p. 31.

biconditional (the result of joining them by 'if and only if') is analytic.[236]

Since cognitive synonymy is thus dependent upon the notion of analyticity, Quine decides to leave aside the problem of synonymy and to renew the search for a suitable notion of analyticity.

Carnap and others have attempted to give an account of analyticity in terms of a "precise artificial language with explicit 'semantical rules'."[237] The idea is that analyticity is easy to specify for artificial languages - the only reason for the difficulty in natural languages is their vagueness. However, Quine points out, even if one considers an artificial language in which all the analytic statements of that language are specified by semantical rules, one is still left with the problem of analyticity. The rules of the language may specify that certain statements are to be analytic for that language, but one is still left with the problem of determining what it is that these statements share. We still need to understand what it is that is being attributed to these statements when they are specified as analytic for that language. As Quine puts it:

> In short, before we can understand a rule which begins 'A statement S is analytic for language L_0 if and only if...', we must understand the general relative term 'analytic for'; we must understand 'S is analytic for L' where 'S' and 'L' are variables.[238]

Semantical rules, thus, do not help us to understand the notion of analyticity, rather they are

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[236] ***Ibid.***, pp. 31-32.

[237] ***Ibid.***, p. 30.

[238] ***Ibid.***, p. 33. There are other uses of semantical rules than the one treated here, but Quine feels these uses to be inadequate in much the same way.

helpful in selecting out those statements of an artificial language that are analytic. They "are of interest only in so far as we already understand the notion of analyticity."[239]

Quine also considers another form of semantical rule which does not use the word 'analytic', rather such a rule "merely stipulates, recursively or otherwise, a certain multitude of statements which, along with others unspecified, are to count as true."[240] (Quine assumes for the purpose of the argument that 'true' is uncontroversial.) On such a view, then, we can specify the analytic statements as those that are "true according to the semantical rule."[241] His complaint is that this explanation is not yet adequate since we have an appeal to the unexplained phrase 'semantical rule'. He says:

> Not every true statement which says that the statements of some class are true can count as a semantical rule - otherwise all truths would be 'analytic' in the sense of being true according to semantical rules.[242]

The phrase 'semantical rule' is in need of clarification as much as the word 'analytic', He says further:

> ...no one signalization of a subclass of the truths of L is intrinsically more a semantical rule than another; and, if 'analytic' means 'true by semantical rules' no one truth of L is analytic

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239 ***Ibid***., p. 36.

240 ***Ibid***., p. 31.

241 ***Ibid***.

242 ***Ibid***.

to the exclusion of another.[243]

Quine sees that similar criticisms can be raised against other proposals and at this point in the “Two Dogmas” paper, he gives up the search for a criterion of analyticity and decides that thus far no one to his knowledge has adequately drawn the distinction between analytic and synthetic statements. He says: “That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith.”[244]

This should not be interpreted to mean that Quine denies that truth has both a linguistic and a factual component,[245] but rather that no one has sorted things out in such a way as to clearly define those statements that are true by virtue of their meanings and independent of any facts (analytic statements) and those that are decided factually (synthetic statements).

It is in the last two sections that Quine presents his D-theoretic point of view (which we discussed in Chapter One). As we indicated earlier, it is in these last sections that he tackles the second dogma, the dogma of reduction.

After abandoning his search for an analyticity criterion, he carries on his investigations in the

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243 ***Ibid***., p. 35.

244 ***Ibid***., p. 37.

245 He says: "It is obvious that truth in general depends on language and extralinguistic fact. The statement 'Brutus killed Caesar' would be false if the world had been different in certain ways, but it would also be false if the word 'killed' happened rather to have the sense of 'begat'." (***Ibid***., p. 36.)

theory of meaning with an examination of the verification theory of meaning since this theory might possess the key to the problems of meaning. He characterizes this theory as holding that "the meaning of a statement is the method of empirically confirming or infirming it."[246] Within such a theory an analytic statement can be defined as the "limiting case which is confirmed no matter what",[247] that is, it is true regardless of the way the world is.

Using the verification theory of meaning, one can also get an account of the cognitive synonymy of statements - "statements are synonymous if and only if they are alike in point of method of empirical confirmation or infirmation."[248] Such a definition would give us an alternative approach to the notion of analyticity - that of defining it in terms of logical truth and synonymy as he indicated near the beginning of "Two Dogmas".

Quine sees a review of the verification theory of meaning as necessary since its acceptance can lead to an account of analyticity. It was this review of verificationism that led to the D-thesis, - a thesis that we have seen runs counter to the verification theory of meaning.

The verification theory of meaning requires a particular relationship to hold between a statement and the world. Quine denies that each statement can be individually confirmed or

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[246] ***Ibid***., p. 37.

[247] ***Ibid***.

[248] ***Ibid***.

Quine indicates that using this concept of synonymy of statements we could get to the cognitive synonymy of the other linguistic forms (see footnote 65 above).

infirmed, but rather "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body."[249] This view derives from the D-thesis: "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system."[250]

As we saw in Chapter One, Quine's view developed into his field theory of knowledge. On the basis of this theory, Quine denies that there is some ultimate difference in kind between analytic and synthetic statements. Hence Quine's field theory relates to the problems in the theory of meaning. If one accepts the D-thesis (and the field theory of knowledge), then not only is the dogma of reduction ("Every meaningful statement is held to be translatable into a statement (true or false) about immediate experience"[251]) too narrow, but also the analytic-synthetic dogma is an over-simple view of the way that language relates to the world. It is not a simple matter of determining a relationship to hold between individual statements and events in the world. It is much more complex and involves the relationship of statements to the system of statements in which they are embedded and then the relationship of that entire system to the world.

Quine's field theory of knowledge, as outlined in Chapter One, ties together the difficulties that bothered Quine in his previous philosophical writings. Meaning becomes a function of the field - chiefly the logic of the system and those sentences that one chooses to hold true come what may.

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249 ***Ibid.***, p. 41.

250 ***Ibid.***, p. 43.

251 ***Ibid.***, p. 38.

Ontology, hence also naming, becomes a function of the conceptual scheme. It is a function of what we choose to import into our language to serve as convenient intermediaries. The choice of ontology, like the field arrangement, is determined by pragmatic considerations such as efficacy, simplicity, elegance, and so on. Even though we have to admit that the field is bounded by experience, truth becomes a function of the system because experience itself comes categorized by concepts. Percepts are replete with concepts.

One expects Quine's theory to sound odd since it runs against a lot that is at the basis of our philosophical-cultural milieu. In fact, this is what makes it difficult to present the D-theoretic viewpoint. In order to communicate with one's peers about the matter, one must use language and concepts to which the D-thesis is hostile. Perhaps, that is why Quine presented his thesis in a metaphorical way in "Two Dogmas". The only way, though, to test the significance of the D-thesis and its peculiar brand of epistemology is to hear the D-theorist out. To use an old expression, "the proof is in the pudding".

CHAPTER THREE

Criticism of the D-thesis

A vast amount of literature was generated in reaction to the views expressed in the "Two Dogmas..." article. Most of this literature was directed to Quine's views on the analytic-synthetic distinction (see Chapter Two), but some of it does pertain to the D-thesis of Quine. It is on this latter material that we wish to concentrate.[252] Most of the material on this topic springs from reactions to Adolf Grünbaum's criticisms of Quine's position.

In this Chapter, we intend to consider first Grünbaum's early criticisms of the D-thesis, - those contained in his book, ***Philosophical Problems of Space and Time***. We shall try to incorporate into this exposition some of the immediate responses to each criticism, as well as some criticisms of our own. After we have completed this mainly expository part on the criticisms of Grünbaum, we shall have a section on the notion of semantic stability. In this section we shall consider the responses that Grünbaum made in his Thalheimer lecture to the various criticisms of his early work. We shall finish the Chapter with criticisms of Grünbaum's later work and an attempt to make more precise the context in which hypotheses are verified and falsified.

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[252] The literature specifically on the analytic-synthetic distinction is certainly relevant to the matter of Quine's D-thesis, but proper consideration of that material would be an entire thesis in itself.

Grünbaum has dealt with the D-thesis in several articles.[253] Typical of his early criticism of

[253] Articles by Adolf Grünbaum pertaining to the D-thesis:

(a) "The Duhemian Argument", ***Philosophy of Science*** 27 (1960) pp. 75-87.

(b) "Law and Conventions in Physical Theory" in ***Current Issues in the Philosophy of Science*** ed. Feigl and Maxwell (New York, Holt, 1961) pp. 140-155.

(c) "The Falsifiability of Theories: Total or Partial? A Contemporary Evolution of the Duhem-Quine Thesis" ***Synthese***, vol. xiv, no. 1 (March, 1962) pp. 17-34.

(d) "Geometry, Chronometry and Empiricism" in ***Minnesota Studies in the Philosophy of Science***, vol. III ed. H. Feigl and G. Maxwell (Minneapolis: Univ. of Minnesota Press, 1962) pp. 405-526.

(e) Chapter 4, "Critique of Einstein's Philosophy of Geometry (A) An Appraisal of Duhem's account of the falsifiability of isolated empirical hypotheses in its hearing on Einstein's conception of the interdependence of geometry and physics," pp. 106-115, (B) "The Interdependence of Geometry and Physics in Poincaré's Conventionalism," pp. 115-131, and (C) "Critical evaluation of Einstein's conception of the interdependence of Geometry and Physics: Physical geometry as a counter-example to the non-trivial D-thesis." pp. 131-151. in ***Philosophical Problems of Space and Time*** (New York, 1963). A.A. Knopf.

(f) "The Falsifiability of Theories: Total or Partial? A Contemporary Analysis of the Duhem-Quine Thesis", in ***Boston Studies in the Philosophy of Science***, ed. M. Wartofsky (Dodrecht; Reidel, 1963).

(g) "Is a Universal Nocturnal Expansion Falsifiable or Physically Vacuous?" in ***Philosophical Studies***, xv, pp. 71-79.

(h) "The Falsifiability of a Component of a Theoretical System", ***Mind, Matter and Method***, Essays in Philosophy and Science in honour of Herbert Feigl. ed. P.K. Feyerabend and G. Maxwell (Minneapolis, 1966) Univ. of Minnesota Press, pp. 273-305.

(i) ***Geometry and Chronometry in Philosophical Perspective*** (Minneapolis, 1968), Univ. of Minnesota Press.

j) "Can We Ascertain the Falsity of a Scientific Hypothesis", ***Studium Generale***, vol. 22, Fasc. 11, (1969) pp. 1061-1093. This was delivered as the Thalheimer Lecture at John Hopkins University on May 9, 1969.

(k) "Can We Ascertain the Falsity of a Scientific Hypothesis" ***Observation and Theory in Science***, ed. M. Mandelbaum (Baltimore, 1971). The John Hopkins Press, pp. 69-129. This is a revised and enlarged version of (j). I will refer to this as the k-version, otherwise I will be referring to (j).

the D-thesis is that found in the fourth chapter of his book, ***Philosophical Problems of Space and Time.***[254] He begins his discussion there with an examination of the claim that there is an asymmetry between the verification of a theory and the falsification of a theory. As Hume and others have pointed out we cannot prove our scientific theories to be true. Verification is inconclusive. A theory is always subject to disconfirmation no matter how highly confirmed it may be. The future may hold a disconfirming instance. Our theories are not deducible from the experimental data that follow from our theories. That is, particular experimental results may support many alternative theories, but it will not provide us with any way of choosing which of the alternatives is the correct one. Thus no number of confirmations can fully verify a theory. Falsification or refutation, on the other hand, is taken to be conclusive.

Even though experiments cannot decisively determine which alternative theory in a set of competing alternatives is true, there is yet hope that science can progress by eliminating the false theories. The notion that falsification is conclusive provides the assurance that there would be some progress in science; through a process of elimination. Once scientists have found a falsifying instance of a theory, they can do either of two things: revise the old theory, in which case it becomes a different theory; - or abandon the old one altogether and look for a completely new theory. Science can thus progress by eliminating those theories that are falsified by experiments, and retaining those theories that are highly confirmed and as yet unfalsified by experience.

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[254] A. Grünbaum, ***Philosophical Problems of Space and Time*** (New York, 1963). A.A. Knopf.

Duhem denied that there is this asymmetry between verification and falsification. He believed that falsification is just as inconclusive as verification. Thus, for Duhem, the traditional view of science progressing by a process of elimination was incorrect.

In order to show Duhem's point. Grünbaum uses simple logic. The assymetry can be presented as follows:

> If a theory T_1 entails observational consequences O, then the **truth** of T_1 does not, of course, follow deductively from the truth of the conjunction
>
> $$(T_1 \rightarrow O) \cdot 0.$$
>
> On the other hand, the **falsity** of T_1 is indeed deductively inferable by **modus tollens** from the truth of the conjunction
>
> $$(T_1 \rightarrow O) \cdot \sim 0.$$ [255]

Obviously, the first is the fallacy of affirming the consequent. A theory entails certain observational consequences, but is not entailed by its observational consequences. The second argument has a valid form, that of **modus tollens**. Thus if a theory entails certain observational consequences and they do not obtain, then the theory is falsified. Grünbaum does not elaborate on what he means by 'theory' or 'observational consequences' or even the entailment that is being used.

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[255] ***Ibid***., p. 106.

Duhem regarded such a characterization to be an oversimplification. If T_1 denotes an individual hypothesis H (Grünbaum does nor specify what an 'individual hypothesis' might be), then O is not deduced from H alone, but rather from the conjunction of H and the relevant body of auxiliary assumptions $A_1, A_2, ..., A_n$ ($n > 0$). Now if we represent the set of auxiliary assumptions by A, then we can see that the falsifiability of H is no longer conclusive. We now have the following schemata:

(i) $[(H \cdot A) \rightarrow O] \cdot O$ (verification)
and
(ii) $[H \cdot A) \rightarrow O] \cdot {\sim}O$ (refutation).[256]

Thus using the **modus tollens** argument it is the conjunction $(H \cdot A)$ that is falsified and not H by itself (i.e., from (ii) we deduce ${\sim}(H \cdot A)$ which is a significantly different result than was deduced in the oversimple case). Under this new schema, the H is not separately falsifiable. Grünbaum says:

> The recognition of the presence of the auxiliary assumptions A in both verification and refutation of H now makes apparent that the **refutation** of H **itself** by **adverse** empirical evidence ~O can be no more decisive than its verification (confirmation) by **favourable** evidence O.[257]

The full implication of this Duhemian view is that:

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256 ***Ibid***., p. 107.

257 ***Ibid***.

> In short, isolated component hypotheses of far-flung theoretical systems are not separately refutable but only contextually disconfirmable: **no one constituent hypothesis H** can ever be **extricated** from the ever-present web of collateral assumptions so as to be **open** to **separate refutation** by the evidence as part of an **explanans** of that evidence, just as no such isolation is achievable for purposes of verification.[258]

Now if T_1 is taken as the entire theoretical system and not as a component hypothesis, then the logical situation is as we find it in the first set of schemata, and it is the entire theoretical system that is falsified.

This Duhemian view corresponds in part to the position expressed by Quine at the end of his "Two Dogmas.." paper. The unit of empirical significance is not the individual statement, but rather the entire theoretical system or the ‘whole of science’. Grünbaum attributes to philosophers like Quine the following claim:

> No matter what the specific content O′ of the prima facie adverse evidence ~O, we can always justifiably affirm the truth of H as part of the theoretical explanans of O′ by doing two things: First, blame the falsity of O on the falsity of A rather than on the falsity of H, and second, so modify A that the conjunction of H and the **revised** version of A′ of A does entail (explain) the actual findings O′.[259]

This is the basic import of Quine's D-thesis: "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system."[260] Thus any statement can be saved from falsification by altering the set of auxiliary assumptions in the appropriate way.

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258 ***Ibid***., p. 108.

259 ***Ibid***.

260 Quine, “Two Dogmas...”, ***op. cit***., p. 43.

We have to be careful to separate Duhem's claims from the claims made by Quine. Duhem's argument, as we have seen, is directed against the asymmetry thesis that an hypothesis can be conclusively falsified, but can never be conclusively verified. He claims that falsification is also inconclusive, since the data never indicate whether the falsity is due to the hypothesis being tested or one (or more] of the auxiliary assumptions. [In effect, whole theories bear the brunt of responsibility for falsification, not individual hypotheses.) Quine follows Duhem in claiming that individual hypotheses are not separately falsifiable, but makes the additional claim that a particular hypothesis can be saved from refutation by making appropriate adjustments in the auxiliary assumptions (the accompanying theory or other aspects of the whole theoretical system.] Grünbaum in his early writings on the subject is not careful to distinguish these two claims, but he does so in his later writings as we shall see later in the chapter.

Grünbaum has three major points to make against Quine's D-thesis. First, he believes that the D-thesis "is true **only** in various **trivial** senses of what Quine calls 'drastic enough adjustments elsewhere in the system'".[261] He feels that "no one would wish to contest any of these thoroughly uninteresting versions of the D-thesis."[262] Secondly, he believes that the non-trivial form, its exciting form, is logically a non-sequitur.[263] It does not follow from anything. He claims that there is no logical guarantee that we can find the required A′, rather "the existence of the required set A′ needs

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[261] Grünbaum, ***Philosophical Problems***... ***op. cit***., p. 110.

[262] ***Ibid***.

[263] ***Ibid***.

separate and **concrete** demonstration."[264] That is, if there is such an A', it needs to be derived for each individual situation. Since there can be no such empirical support, Grünbaum sees the D-thesis as "an unempirical dogma or article of faith."[265] Thirdly, Grünbaum believes that he has a counter-example which shows that the non-trivial D-thesis is false. He thinks that he can separately falsify a particular component hypothesis. Now certainly if Grünbaum succeeds in producing such a counter-example, then he would have removed any possibility of a logical proof of the D-thesis and would have placed the search for appropriate A' permanently in the realm of the empirical; - that is, one would have to find the appropriate revision for each individual case since there would be no logical assurance that one exists for every case.

Before we consider each of these criticisms in turn, we should point out that we object to the rather loose way that Grünbaum has presented the matter. First, it is not entirely clear that scientific experiments are conducted in the way that Grünbaum's exposition indicates. It may be that a proper description of the way in which experiments are conducted will eliminate part of the dispute. It must be made clear just what T_1 is and what O is. More particularly, it should be made clearer what is meant by saying that O is a consequence of T_1. What does '$T_1 \rightarrow O$' mean? Also it is not exactly clear what ~O is.

For the sake of the discussion, let us assume that Grünbaum intends T_1 to stand for the set of statements of a given theory. Without getting involved in the dispute over experimental laws and

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264 ***Ibid***.

265 ***Ibid***.

theoretical laws[266] let it suffice to characterize these statements as consisting of the laws of the theory and all the mechanisms that permit one to move from the laws to given statements that are somehow testable through experience. Now O is taken to be an observational consequence of the theory. It will be a statement that says something is the case. For example, when water is heated to a temperature of 212°F it boils under standard conditions. The O in such a case may be "The water boils". O is said to be an observational consequence of T_1 if it is the case that given all the mechanism of the theory one can derive O from the statements of the theory and the statements of certain conditions. O is an observation statement.

Now if the state of affairs reported by the statement O does not obtain, then we can say that it is not the case that O. Thus we take ~O to mean that the state of affairs, described by the statement O, does not obtain. So throughout we are concerned with certain relationships that obtain between statements except in the case of evaluating our observation statements in which case we are concerned with a relationship that is between a statement and a state of affairs (We will discuss this in greater detail later on in the thesis).

The Trivial Case of the D-thesis

Grünbaum presents two trivial cases which can be passed over as being indisputably trivial and uninteresting . Both are cases where the actual content of the individual hypothesis H does not enter into the explanation of O. The first case is the one where the revised A′ chosen is O′, the statement which describes the state of affairs that obtains. In such a case we have $(H \cdot 0') \rightarrow \sim 0'$, -

266 E. Nagel, ***The Structure of Science*** (New York, 1961). Chapter five.

that is, $0'$ follows from $(H \cdot 0')$ by some form of simplification. It does not matter whether the first conjunct is H or any other statement. This kind of case can be blocked by. insisting that H be needed in the deduction of O'. In our second case this condition is fulfilled and still we have a trivial case. This is a case where A' has the form $(\sim H \vee O')$, (-the usual definition for material implication, $(H \supset O')$). In such a case O' will follow by some form of disjunctive syllogism.

A student of Professor Grünbaum, Philip Quinn, has suggested that the first case can be blocked by requiring "that H be relevant to the deduction of O'."[267] It is not entirely clear whether or not such a requirement will exclude such a case as the second case, for certainly, in one sense, the H is needed for the deduction, but the entailment relation could be selected in such a way that $(H \cdot (\sim H \vee O')) \rightarrow O'$ is not a tautological entailment.[268] It is likely that both cases can be blocked by insisting on a fairly strong entailment relation; that is, by insisting that if Y is to be deductible from

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[267] P.L. Quinn, "The Status of the D-thesis", ***Philosophy of Science*** XXXVI, 4, (1969) p. 387.

Quinn also insists on a condition "that A' be a theoretical statement" (p. 387), but there is a difficulty here since if one accepts the D-thesis there is no absolute distinction between theoretical statement and observation statements. We discuss this later in the thesis.

[268] ***Ibid***.

Quinn's paper in the ***Philosophy of Science*** is the same as Chapter Four in his dissertation for the University of Pittsburgh (1970) entitled ***Duhemian Conventionalism***. In the Introductory Remarks to the dissertation Quinn indicates that the entailment operator that he employs "bears some resemblances to the concept of entailment embodied in the system E of Anderson and Belnap." (pp. 2-3). It is also interesting to note that Grünbaum intends an entailment that is stronger than material implication. Quinn refers us to p. 162 of Grünbaum's paper "Law and Convention in Physical Theory", in ***Current Issues in the Philosophy of Science***, ed. H. Feigl and G. Maxwell (New York, 1961). Quinn says: "Although Grünbaum does not give a positive characterization of the notion of entailment, it is plausible to suggest that when he says T_i is used in the explanation of O' he means at least that T_i must not only appear in the explanation of O' but also must appear relevantly, i.e., must be a premiss whose content is actually used in the deduction of O'." (Quinn, ***Philosophy of Science***, pp. 386-7.)

X, it not only must be the case that $(X \cdot \sim Y)$ is logically false, but also there must be some connection between the meaning of X and the meaning of Y. Quinn has suggested an entailment like that found in Anderson and Belnap's system E.[269]

These trivial cases seem to be relatively uncontroversial. More controversial (according to the literature in the area) is Grünbaum's attempt to classify certain other cases as trivial applications of the D-thesis. The trivial cases already considered above could not be considered as "drastic enough adjustments elsewhere in the system." (Recall that Grünbaum's first point was that the D-thesis "is true **only** in various **trivial** senses of what Quine calls 'drastic enough adjustments elsewhere in the system.'") What can be construed as a "drastic enough adjustment" is a case in which one uses a meaning change in order to save a particular hypothesis. Grünbaum also classifies such a move as trivial. His example is the following:

> For if someone were to put forward the false empirical hypothesis H that "Ordinary buttermilk is highly toxic to humans," this hypothesis could be saved from refutation in the face of the observed wholesomeness of ordinary buttermilk by making the following "drastic enough" adjustment in our system: changing the rules of English usage so that the intension of the term "ordinary buttermilk" is that of the term "arsenic" in its customary usage.[270]

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[269] Note that if our entailment is to block cases of the second type then the following,

$$(H \,\&\, (\sim H \vee O')\,) \rightarrow O'$$

> cannot be a tautological entailment. I have not investigated to see whether or not such a theorem holds in Anderson and Belnap's system E. If it does then their system would need to be further restricted.

[270] Grünbaum, ***Philosophical Problems*** ... ***op. cit***., p. 111.

This, then, must be one of the "thoroughly uninteresting versions of the D-thesis" that "no one would wish to contest." Presumably, he is saying that given that you allow such a semantical shift, then it is uninteresting to debate the D-thesis. Grünbaum, therefore, demands that "a **necessary** condition for the non-triviality of Duhem's thesis is that the **theoretical language** be **semantically stable** in the relevant respects."[271]

If one considers Grünbaum's buttermilk example, one has to agree that such a shift trivializes the D-thesis. However, certain critics of Grünbaum have resisted his necessary condition for the non-triviality of the D-thesis. We deal with this question in some detail later in this chapter.

If we follow Grünbaum's presentation in ***Philosophical Problems of Space and Time***, he next admits that he is "unable to give a formal **and** completely general **sufficient** condition for the **non**-triviality of A'."[272] J.W. Swanson shows that there are infinitely many syntactical strategies "for producing the desired A'."[273] He says:

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[271] ***Ibid***.

[272] ***Ibid***., p. 112.

[273] J.W. Swanson, "Discussion of the D-thesis", ***Philosophy of Science***, vol. 34, no. 1. (1957) p. 63.

Swanson shows us:

"The situation is as follows. One has a first order theory T_0 consisting of the axioms H, A_1, A_2, ... , A_n such that some unexpected and unwanted consequence O is deducible from T_0 (i.e. $T_0 \vdash 0$). Now take any consistent first order theory K with sole observational consequence, O', and all non-observational constants (i.e., all theoretical predicate and individual constants) distinct from those of T_0 and such the $T_{0'} \cup K$ is consistent, where $T_{0'}$ is got from T_0 by removing the axioms of T_0 one or more of A_i found in the proof H, A_{i1}, A_{i2}, ..., $A_{ik} \vdash 0$. If other observational consequences O_1, O_2, etc. of T_0 are lost by this move, either adjoin them outright as part of K, or let K include formulae A_1, A_2 etc., and formulae $A_1 \rightarrow O_1$, $A_2 \rightarrow O_2$ etc. assuring the provability of O_1, O_2, etc., in

> ...on the strictly logical, syntactical level, it simply does not make sense to speak of the non-existence, or even of the triviality of revised A (i.e., A′) such that (H · A′)→ 0′. There do exist such A′ (indeed, infinitely many of them) and they may be as complex as you please.[274]

Such considerations show the difficulty in presenting a formal and completely general sufficient condition for the non-triviality of A′. The only problem with Swanson's strategies is that interpretations would be required for his uninterpreted calculi in most applications of the D-thesis and so the question remains whether such interpretations can be found.

Next Grünbaum passes to the question of resorting to non-standard logics in order to sustain the D-thesis. Here he is referring to Quine's claim that a given hypothesis can be held true "by amending certain statements of the kind called logical laws."[275] He complains that "the invocation of non-standard logic either makes the D-thesis **trivially** true or turns it into an interesting claim which is an unfounded dogma."[276] His complaint is related to his demand for semantical stability since he is objecting primarily to a change in meaning. He writes:

> For suppose that the non-standard logic used is a three-valued one. Then even if it were otherwise feasible to assert within the framework of such a logic that the particular statement H is "true", the term "true" would no longer have the meaning associated with the two-valued framework of logic within which the D-thesis was enunciated to begin with.[277]

K. Thus O is no longer provable, O' **is** provable, and $T_{O'} \cup K$ is the desired reconstruction of T_0. Now the complexity of K has not been specified. It might be a theory of vast intricacy."

274 ***Ibid***., p. 63.

275 Quine, "Two Dogmas..." ***op. cit***., p. 43.

276 Grünbaum, ***Philosophical Problems***..., ***op. cit***., p. 112.

277 ***Ibid***.

One cannot deny that the meaning of "true" will be changed from the one system to the revised system. That is the whole point of changing the logic. That is what makes them different systems.

Grünbaum ranks this kind of move along with the semantical shift. He says: "It is not to be overlooked that a form of the D-thesis which allows itself to be sustained by alterations in the meaning of "true" is no less trivial **in the context of the expectations raised by the D-thesis** than one which rests its case on calling arsenic 'buttermilk.'"[278] We shall leave aside the issue of alternative logics and concentrate later on Grünbaum's necessary condition of semantic stability since Grünbaum sees the issues as tied together.

Before we consider Grünbaum's non-trivial cases and later debate the issue of his necessary condition of semantic stability, we must determine just what Grünbaum is claiming in his 'buttermilk' example. Another of his examples helps to do this. This example turns up near the end of the section entitled "The Trivial Validity of the D-thesis" in the fourth chapter of ***Philosophical Problems of Space and Time***.

Grünbaum imagines a situation where we have two substances, I_1 and I_2, which are of exactly the same composition except the arrangement of the atoms in the molecules is different. I_2 is highly toxic, whereas I_1 is not toxic. I_1 is called "duquine". The hypothesis, H that is to be saved is "Duquine is highly toxic."

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278 ***Ibid.***, pp. 112-3.

As in the 'buttermilk' example, the H is saved by changing the meaning of a word, in this case "duquine". The intension of "duquine" is changed slightly to connote I_2. Such a move is what Grünbaum would call a trivial application of the D-thesis. He is careful, though, to indicate what he considers to be trivial in this example. He writes:

> The preservation of H from refutation in the face of evidence by a partial change in the meaning of "duquine" is trivial in the sense of being only a **trivial** fulfilment of **the expectations raised by the D-thesis**. But, in my view the possibility as such of preserving H **by this particular kind of change in meaning** is not at all trivial. For this possibility as such reflects a **fact about the world**: the existence of isomeric substance of radically different degrees of toxicity...[279]

We can, therefore, take Grünbaum to be saying that: given such a semantic shift is possible, then the D-thesis trivially follows, and that this is so is not very interesting or exciting. However, he qualifies this by saying that the possibility of this kind of semantical shift is not trivial at all since it involves the non-semantical consideration of a state of affairs in the world, In other words, such a shift in intension is made possible because of the way things are arranged in the world.

This is well and good, but the matter is still unclear. Granted, the D-thesis follows trivially if such a semantic shift is possible. This aspect Grünbaum finds uninteresting. What he finds more interesting is the possibility of such a semantical shift for it reflects a fact about the world. In contrast, though, we do not find this very exciting at all simply because the world is diverse enough that such a semantic shift will always be possible for a D-theorist with the least amount of

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[279] ***Ibid***., p. 113.

imagination. The more important issue is whether or not it is of any use to make such semantic shifts. In the ‘buttermilk’ example where the hypothesis being saved was "Ordinary buttermilk is highly toxic to humans", one needed only to find some highly toxic substance like arsenic and claim to revise your language so that ‘buttermilk’ refers to that substance. This shift, too (as in the case of “duquine”), reflects **a fact about the world** - namely that there is a substance in the world that is highly toxic to humans. In both cases the process of renaming the substance is a trivial application of the D-thesis. In the buttermilk example one might even be able to save the hypothesis without a semantic shift by arguing that if a human being consumed enough (vast quantities of) buttermilk it could kill him.

What is interesting is not that there is a corresponding fact in the world (- a D-theorist would be foolish to deal with some imaginary situation which could not be tested in the actual world - in such a case his proposal to save the hypothesis would be rejected very quickly), but rather the reasons for advocating such a change in semantics? These reasons must not only reflect the state of affairs, but must also be convincing enough so that the change be adopted by the scientific-cultural milieu (or at least given serious consideration for adoption) within which the hypothesis-tester is working. There is no point in altering semantics in order to save a pet hypothesis unless it results in a significant pragmatic change for the science involved (- such as a more efficient science, a simpler explanation, a more elegant presentation or some significant innovation).

In both of the examples considered, the ‘buttermilk’ example and the ‘duquine’ example, a respectable D-theorist would not advocate such moves. These moves are far from creating a better science; they tend to gloss over important differences found in the world - buttermilk is significantly different from arsenic and I_1 and I_2 have different molecular structures. Both are silly applications of the D-thesis and for pragmatic reasons would never be considered by a serious D-theorist. However, there are, as we shall see, cases where semantical shifts are employed which are more controversial. These cases make one wonder what significant difference is underscored by classifying applications of the D-thesis as trivial or non-trivial. Grünbaum is unable to provide "a formal **and** completely general **sufficient** condition for the **non**-triviality of a'",[280] and, as we shall see, his necessary condition of semantical stability breaks down. Perhaps, a better classification would be between those applications of the D-thesis that are useful and those that are useless. Some cases of semantical shift are useful and others are obviously useless. The earlier trivial cases and these latter cases that Grünbaum invented come under the latter classification.

The Non-Trivial Case

Grünbaum considers the D-thesis in its non-trivial sense to be a non-sequitur, a dogma, and an article of faith. His claim is that the fact that H cannot be found to be false from the premise $[(H \cdot A) \rightarrow 0] \cdot \sim 0$ by some rule like Modus Tollens "is quite insufficient to show that H can be preserved non-trivially as part of an explanans of any potential empirical findings O′"[281] It is true that this does not show that H can be non-trivially preserved under every adequate definition of non-trivial. That there is such a non-trivial A′, call it 'A'_{NT}', is a separate claim. It does not follow

[280] ***Ibid***., p. 112.

[281] ***Ibid***., p. 114.

that just because H cannot be separately falsifiable, that $(\exists A'_{NT})\ [(H \cdot A'_{NT}) \rightarrow O']$, but it also does not follow that there cannot be such a non-trivial A', i.e. A'_{NT}.

Duhem's claim is not as strong as Quine's. For him, no individual hypothesis can be separately falsifiable, whereas the D-theorist makes this claim and in addition claims that a non-trivial A' can be found such that $(H \cdot A')$ entails O'. Thus all that Grünbaum has done in this criticism is draw attention to two independent claims of Quine's D-thesis. Philip Quinn has put forward a proof that shows these two claims to be independent (see footnote 96).

It is premature for Grünbaum to claim that "the existence of the required non-trivial A' would require **separate** demonstration for each particular case."[282] That statement certainly needs to be proven before it can be claimed as true. As Swanson points out:

> Given any H and any O', how could one be guaranteed that there is **not** forthcoming some A'_{NT} such that in the new calculus "$(H \cdot A'_{NT}) \rightarrow O'$" holds, and such that an interpretation (a model) can be found to satisfy the calculus?[283]

Grünbaum must present an air-tight counter-example if he is to demonstrate that there can be no logical proof of the D-thesis and that the existence of the required A'_{NT} must be derived for each individual situation.

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282 ***Ibid***., p. 115.

283 Swanson, ***op. cit***., p. 64.

Grünbaum's Falsifying Counter-Example

Grünbaum believes that he has a 'falsifying' counter-example. In an article which is a revised version of the sections in ***Philosophical Problems of Space and Time***, he says that he intends to "adduce logically possible empirical findings O′ for which every non-trivial A′ that is capable of preserving H in the face of 0′ is empirically false."[284] If he can do this, then he will have shown the H to be separately falsifiable. This would show the D-thesis to be false since any A'_{NT} which may be conjoined to H to give the assumedly true O′ must itself be false and hence there would be no way of preserving H truly in the face of the evidence O′. Any true H which is conjoined to a true A'_{NT} will therefore yield observational consequences that are incompatible with the observed O′ and H will be separately falsifiable since the falsity can only be attributed to H.

Fortunately, for the D-theorist, Grünbaum does not fulfil his intentions, and as we shall show later on, he cannot fulfil his intentions.

Grünbaum's 'falsifying' counter example arises from Einstein's claim about the interdependence of geometry and physics.

> Einstein argues that the geometry itself can never be accessible to experimental falsification in isolation from those other laws of physics which enter into the calculation of the correction's compensating for the distortions of the rod.[285]

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[284] Grünbaum, "The Falsifiability ..." ***op. cit.***, (see footnote 2(h)) p. 281.

[285] Grünbaum, ***Philosophical Problems***..., ***op. cit.***, p. 132.

The rod referred to is the rigid solid rod used by physicists as the physical standard for congruence in the determination of the geometry. From this, Einstein concluded that "you can always preserve any geometry you like by suitable adjustments in the associated correctional physical laws."[286]

We must be careful how we state Einstein's claim so that it comes out as a form of the D-thesis. Pure geometries are not usually described as true or false in isolation from their applications (interpretations). Quinn is careful in this respect and sets up the counter-example as follows:

> Suppose that we have hypothesized that a certain region of physical space R has geometry G and have made measurements with physical rods to test this hypothesis.[287]

Under this construal, Einstein's claim is more like the claim that one can always preserve a particular geometrical description by making suitable adjustments in the associated correctional physical laws. Thus, an hypothesis that may be either true or false is being put forward and preserved and not some pure unapplied theory that is merely an arrangement of symbols and hence not something that could be either true or false.

Grünbaum sees Einstein's claim as a geometrical form of the D-thesis. His counter-example is an attempt to show the unsoundness of Einstein's claim. First, he deals with "the special case in which no deforming influences are effectively present in a certain region whose geometry is to be

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[286] ***Ibid***.

[287] Quinn, "The Status ..." ***op. cit***., pp. 388-9.

ascertained."[288] Afterwards, Grünbaum makes a critique of "Einstein's Duhemian argument as applied to the empirical determination of the geometry of a region which **is** subject to deforming influences."[289] We shall consider the special case first.

The Special Case:

In this case the correctional physical laws cannot function as auxiliary assumptions. Thus, according to Grünbaum, the geometrical description itself is falsifiable since there is nothing else to which the blame can be attached. The question arises, however, whether one can, in fact, claim that a region is "effectively free from substance-specific deforming influences"[290] without relying on various kinds of collateral theory. Grünbaum claims that you can. He thinks that it would be easy to certify a region as free from perturbations. It would simply be a matter of showing that two solid rods of different composition coincide at every point within the region.[291] However, this is certainly questionable.

At the basis of Einstein's claim is the following argument:

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288 Grünbaum, "The Falsifiability..." (see footnote 2(h)) ***op. cit***., p. 285.

289 Grünbaum, ***Philosophical Problems***... ***op. cit***., p. 130.

290 Grünbaum, "The Falsifiability..." (2(h)) ***op. cit***., p. 286.

291 Grünbaum, ***Philosophical Problems*** ... ***op. cit***., p. 136.

> "For quite independently of the conceptual elaboration of such physical magnitudes as temperature, whose constancy would characterize a region free from deforming influences, the absence of perturbations is certifiable for the region as follows: two solid rods of very different chemical constitution which coincide at one place in the region will also coincide everywhere else in it (independently of their paths of transport)." (***Ibid***.)

1. Physicists use rigid solid rods as the physical standard of congruence in order to determine the geometry of a space.
2. Just like any physical substance, solid rods are subject to deformations as a result of certain physical conditions. For example, metals expand when heated.
3. If the rods are to be used as congruence standards, then corrections have to be made to compensate for the chemical responses of the rod.
4. Thus the question of coincidence becomes a function of the chemical composition of the rods used.
5. Thus the corrections are an essential part of the test for the geometry of the region.
6. Therefore, the geometrical description cannot be isolated in order to be separately falsified.
7. Thus he concludes that "you can always preserve any geometry you like by suitable adjustments in the associated correctional physical laws."[292]

In other words, a geometrical hypothesis can be saved from falsification by blaming whatever laws of physics enter into the corrections that need to be made before one can declare that the rods

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292 Grünbaum, "The Falsifiability ..." (2(h)) ***op. cit***., p. 283.

> "... in order to follow the practice of ordinary physics and use rigid solid rods as the physical standard of congruence in the determination of the geometry), it is essential to make computational allowances for the thermal, elastic, electromagnetic, and other deformations exhibited in solid rods. The introduction of these corrections is an essential part of the logic of testing a physical geometry. For the presence of inhomogeneous thermal and other such influence issues in a dependence of the coincidence behaviour of transported solid rods on the latter's **chemical composition**. Now, Einstein argues that the geometry itself can never be accessible to experimental falsification **in isolation from** those other laws of physics which enter in to the calculation of the corrections compensating for the distortions in the rod. And from this he then concludes that you can always preserve any geometry you like by suitable adjustments in the associated correctional physical laws." (***Ibid***.)

coincide. Instead of blaming the geometrical description in the face of disconfirming evidence, the blame can be shifted to the process by which one determines the absence of perturbations.

Grünbaum admits that "the observational certification of two solids as coinciding under transport and as (quite) different chemically is theory-laden."[293] The question is whether it is theory-laden "to an extent precluding the separate falsifiability of H."[294] Grünbaum's claim, of course, is "that the theory involvement in the claim A that there is freedom from deforming influences is **not** such as to allow the assertion of the kind of A′ needed to deny the separate falsifiability of H by O′."[295]

Grünbaum’s claim is that however theory-laden the observational certification of freedom from deforming influences may be, there is sufficient stubborn fact to disallow any A'_{NT} from saving the H in question. Grünbaum is mistaken. His admission that it is theory-laden opens the door for a D-theoretic defence. We shall see later on that because of this Grünbaum’s particular attempt to present a counterexample must fail.

Counter-examples to Grünbaum’s Counter-example:

Several counter-examples have been derived to show how one might be able to devise an appropriate A'_{NT} to save the geometrical hypothesis. One such counter-example devised by Swanson

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293 ***Ibid***., p. 287.

294 ***Ibid***.

295 ***Ibid***.

is developed from the claim that the **identity** of the physical rods is theory-laden (and not the assertion that the chemical difference of the two rods is theory-laden, as Grünbaum would have it).

As a counter-example, Swanson considers a universe which consists of three different rods a_1, a_2, a_3. In this universe, all pairs that coincide at one place coincide everywhere else. In this universe a_1 and a_2 appear to be identical, but a_1 clearly differs from a_3 and a_2 clearly differs from a_3. The anti-Duhemian, (G), claims along with Grünbaum that the region is free from deforming influences. On the other hand, the Duhemian, (S), claims that all three rods are different from one another, and hence contradicts the claimed identity between a_1 and a_2. In order to account for the apparent identity of a_1 and a_2 at every place in the universe, the Duhemian postulates that there are two different deforming influences in the universe D_1 and D_2. Under the influence of D_1, a_1 expands (or shrinks) by the increment k, but a_2 and a_3 are not affected. However, a_2 and a_3 are deformed by D_2 in the same way that a_1 was deformed by D_1; a_1 is unaffected by D . From this latter point of view, a_1 is no longer identical to a_2 because each is affected differently by D_1 and D_2. Since the effects of D_1 and D_2 neatly cancel each other out, the coincidence of the rods is preserved.[296]

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[296] Swanson, ***op. cit***., p. 65.

> "Consider the model universe U consisting of exactly three rods a_1, a_2, and a_3. Two philosophers, G the anti-Duhemian and S the Duhemian, observe the behaviour of the rods under transport in a certain region. All pairs of rods "which coincide at one place in the region... coincide everywhere else in it (independently of their paths of transport)." Both G and S observe that a_1 and a_2 appear to be identical under all test procedures currently available to physics. But a_1 and a_3 are clearly different from one another (they smell and taste differently), as are a_2 and a_3. Given this evidence, G asserts that the region in which all pairs of rods coincide under transport is obviously free of deforming influences. But S proposes the following explanation of the data. Instead of assuming that the **prima facie** identical rods a_1 and a_2 are really identical S supposes them to be "isomeres". Thus, all three rods are different from one another on S's hypothesis. The three pairs

What Swanson's counter-example shows is that one cannot decide on the basis of the data which explanation is correct, the anti-Duhemian or the Duhemian. Swanson grants that the anti-Duhemian theory is the simpler of the two theories. However, the anti-Duhemian, G, can never be sure that "his data did not mask deforming influences of the kind considered in the model."[297] The fact that such deforming influences might be present shows that one cannot conclusively verify the thesis that a given region is free from deforming influences. Therefore, it is open for the D-theorist to blame something other than the geometry for the conflicting observations.

$$a_1, a_2$$

$$a_2, a_3$$

$$a_1, a_3$$

are all non-identical pairs. But S further postulates for the region in question two rather peculiar deforming influences D_1 and D_2. Think of them as functions with elements of U for domain and a subset of the real numbers for range. The domain of both D_1 and D_2 is the set $\{a_1,a_2,a_3\}$, and their range the set $\{0, k\}$. Here is a complete characterization of these functions:

$$D_1(a_1) = k \quad D_2(a_1) = 0$$

$$D_1(a2) = 0 \quad D_2(a_2) = k$$

$$D_1(a_3) = 0 \quad D_2(a_3) = k.$$

As you can see, D_1 expands (or shrinks) a_1 by an increment k, but has no effect on a_2 and a_3; and D_2 does not affect a_1, but has the same effect on a_2 and a_3 that D_1 has on a_1. Clearly $D_1 \neq D_2$. Moreover, on this hypothesis it is evident that $a_1 \neq a_2$, since they are affected differently by the same deforming influences. Moreover, as one can check in an instant, these two deforming influences preserve nicely the coincidence of the rods, "cancelling one another out", as it were." (***Ibid***.)

[297] ***Ibid***.

Philip Quinn has argued that "Grünbaum has not proved that the assumption that the region is free of differential deforming forces is conclusively verifiable,"[298] and so he does not support Grünbaum's counter-example. He does, however, object to Swanson's argument. He claims that the argument has no relevance to the issue of the independent verifiability of the assumption that a region is free of differential deforming influences.[299] What he questions, though, is not the issue of the relevance of the argument, but rather the issue of how the influences which are postulated by Swanson's Duhemian, S, for the deformations "could qualify as genuine differential deforming forces."[300] The emphasis is on the word 'genuine'. Certainly what Quinn wants to argue is that the argument is wrong, not irrelevant. He claims that D_1 and D_2 are not genuine forces. If this is so, then S's explanation cannot function as an alternative to the assumption of the deformation free region. Swanson, however, could respond by questioning this notion of **genuine forces**. What is a genuine force?

Quinn grants that "Swanson's S is perfectly free to decompose his postulated 'universal deformation' into a mathematical sum of 'differential deformations' which 'preserve nicely the coincidence of the rods cancelling one another out as it were (!)'"[301] However, he claims that this mathematical decomposition is "physically empty"[302] since the universal deformation is metaphorical

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[298] Quinn, ***op. cit***., p. 392.

[299] ***Ibid***.

[300] ***Ibid***.

[301] ***Ibid***.

[302] ***Ibid***.

(i.e., "merely metaphorical in the sense that to assert that all rods suffer them signifies simply that we assign different lengths to all rods in different positions and/or orientations.")[303] The question raised by Quinn is the old metaphysical problem of what is real (genuinely real). Certainly S's postulation of forces corresponding to the magnitudes given by D_1 and D_2 serves as a genuine logical alternative to the thesis of a deformation-free region. The reality of such forces is another question. Grünbaum's counter-example was only to show that a case can be constructed or is logically possible where an individual hypothesis is separately falsifiable. Swanson's response was to show that it was not the only logically possible description, but rather it is just as logically possible to reconstruct the situation in a way that is favourable to the D-thesis. It is granted that G's theory is a simpler one and for that reason it is more apt to be selected by scientists choosing between the two alternative theories. However, if a scientist strongly wished to retain the geometric description as Euclidean and could do so only by postulating such an "universal deformation", then he may very well opt for S's thesis. This is what the D-theorist is claiming. Einstein, for example, if he wished to preserve his particular geometrical description could select parameters in much the same way that Swanson's S does in the above example. The selection of the parameters would be governed by the changes required to retain the Euclidean description. Such a move may be forbidden by claims about simplicity, efficiency, elegance, and so on, but it is not logically forbidden. The question of what is real does not enter in at this point but later on, for it is the whole theory that is to be judged. Competing theories will each be set up in such a way that they explain the data(i.e., to mirror reality). Each competing theory will create its own way of looking at the world - it determines its reals. The question is which theory does it in the best way, and this is where the pragmatic factors enter. The

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303 ***Ibid***.

point is that the question of genuineness does not enter in at this point of the discussion, and that Quinn's criticism is irrelevant. (We return to the role of the pragmatic factors later).

The D-thesis asserts that one can retain an hypothesis come what may. Of course, one must suffer the consequences of such a choice. This may mean suffering unwieldy 'universal deformations' in order to retain a particularly elegant and familiar geometrical description. Such are the ways of science, there are no 'genuine reals', but rather we use what we can to cope efficiently with the world. (One thinks of such posits as electrons, molecules, and so on - Quine even considers tables, chairs and other such ordinary physical things as posits (see our Chapter Two, particularly the parts on "to be is to be the value of a variable") - a language creates its own reals).

Although Quinn argues that Swanson's counter-example does not suffice to show that the assumption that there are no differential deforming forces present is not separately verifiable,[304] he does think it suggests "an interesting strategy."[305] He revises Swanson's procedure and presents a strategy[306] which illustrates the kind of procedure that Einstein could use to preserve

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304 ***Ibid***., p. 393.

305 ***Ibid***. Quinn finds it "an interesting strategy for vindicating subthesis D_2 with respect to H, the hypothesis that the geometry of the region R is G."

306 Quinn has revised the Swanson strategy as follows:

> Consider the following Swansonian recipe for finding an A'_{NT} to be called the revised Swansonian procedure or RSP:
>
> (1) Find the position-independent - I neglect orientation dependence for the sake of simplicity - expansion or contraction of all rods which if postulated as a Reichenbachian "universal deformation" would allow the retention of H in the region in question; call the function expressing it, schematically. if f(x), where the position x is specified independent

of metric;

(2) Define deformation functions $f_1(\underline{x})$ and $f_2(\underline{x})$ such that in the region R for all positions $f_1(\underline{x}) = f_2(\underline{x}) = f(\underline{x})$, but outside R there is at least one position such that $f_1(\underline{x}) \neq f_2(\underline{x})$;

(3) Propose as an auxiliary assumption A′ the hypothesis that rods of chemical kind K, where kind K is taken from the antecedently given chemical theory, e.g., iron, are subject to the differential deformation specified by f_1 and all other rods are subject to the deformation specified by f_2.

I claim that an A′ obtained by applying RSP satisfies all the conditions necessary to make it a non-trivial revision of A and, since A′ can also be held to be true such an A′ satisfies the condition of subthesis D_2 with respect to H and the observations in question. In the first place, the fact that the superposition of f_1 and f_2 inside R has the same effect as f guarantees that observations that refuted H & A will be explained by H & A′. In the second place, f_1 and f_2 do affect chemically different substances differently, f_1 affecting only substances of kind K, and their effects differing at some position outside R; this insures that these deformations are genuinely differential. The fact that the difference between f_1 and f_2 does not manifest itself inside R is of absolutely no importance. Thirdly, it is always possible to postulate the existence of unknown forces having genuine physical sources as the agencies responsible for the deformations specified as f_1 and f_2 since there is no reason to suppose that scientists have yet discovered all the perturbing influences which exist in nature. And, finally, the conjunction H & A′ is as falsifiable as the conjunction H & A was, for if we are given f_1 and f_2 and determine that at position $\underline{x}_0$ outside of R $f_1(\underline{x}_0)$ and $f_2(\underline{x}_o)$, then we have only to transport a rod of kind K and a rod of some other chemical kind which coincide initially in R to $\underline{x}_0$. If these rods fail to coincide as predicted in accordance with A′ then A′ is to some extent confirmed. If they continue to coincide, then the conjunction H & A′ has been refuted. In this case we might wish to avail ourselves of RSP to construct an A′′ using functions f1' and f2' which are equal in R and at $\mathbf{x_0}$ and which differ at some other position. And, of course, this procedure can be repeated as often as needed. Hence, subthesis D_2 can be satisfied for H and any finite set of observations.

Furthermore, the repeated use of RSP can proceed without challenging the standard theories of chemical kinds. It thus requires the use neither of Swanson's hypothesis that **prima facie** chemically identical rods are really different nor of the hypothesis that **prima facie** chemically different rods are really identical. Hence, RSP is not affected by Grünbaum's warning that to claim chemical difference to be sufficiently theory-laden to save H from refutation is to introduce an intolerable ambiguity into scientific testing. Indeed, the only restriction of the use of RSP seems to be the following: If one wishes to assert that, the differential deformations introduced in A′ are the effects of real physical influences rather than a result of covert stipulation about the lack of self-congruence of differentially unperturbed rods, one must be willing to postulate the existence of physical sources for the forces which cause the deformations. But, from a logical point of view, this can always be done since such existential assertions are by themselves irrefutable. Thus, although the repeated application of RSP may be scientifically otiose one may always resort to it to satisfy subthesis D_2 with respect to H and any finite set of observations. (Quinn, ***op. cit***. pp. 393-4.)

Quinn also mentions a thesis of Lawrence Sklar which parallels the Swanson thesis. (Lawrence Sklar, "The Falsifiability of Geometric Theories," ***The Journal of Philosophy***, vol. 64., (1967) pp. 247-253.) Sklar's procedure also provides a method for finding a requisite A'_{NT}. Quinn tells us that RSP has the advantage over Sklar's procedure of insuring "that chemically different rods will be differently affected whereas SP merely guarantees that some two classes of rods or other will be differently affected." (Quinn, ***op. cit***., p. 395.) Since Sklar's procedure or SP so closely resembles Swanson's, we shall leave aside a detailed examination of it as superfluous, and redundant.

Quinn reports that:

> "Sklar wants to provide a recipe for finding a "differential metrical force field" such that the A′ describing this "field" can be used to show that H (in Sklar's discussion, the particular hypothesis that the geometry of the region R is Euclidean) satisfies subthesis D_2. Sklar's procedure for cooking up the requisite "field," call it SP, goes as follows:
>
>> (1) Find the remetrization shrinking or lengthening of measuring rods upon transport in a manner dependent only upon location nonmetrically specified) which, if postulated as a "universal force" in the sense of Reichenbach, would allow us to retain our hypothesis of the Euclidean nature of the space in question. By Grünbaum's own contention, this is always possible. In fact he considers it a vital fact about space that such a "conventionality" is available in respect to it.
>>
>> (2) Find a property, specifiable in purely general terms, shared by all the measuring rods used in the metrical experiments in question, but **not** shared by all the actual or possible measuring rods in general.
>>
>> (3) Postulate the existence throughout the volume of space in question of a "force" (field capable of lengthening or shrinking rods upon transport) identical with the "universal force" of (1) for rods possessing the property found in (2), and identical with the zero "force" (no lengthening or shrinking upon transport) for all measuring rods not possessing the distinctive property. (Sklar, p. 251).*
>
> Sklar claims that the general property required by (2) can always be found., if necessary, it can always by a very unentrenched "Goodmanian" property such as being a measuring rod used in the **n**th series of metrical experiments carried out by human beings. He also claims that the theory, call it T, composed of the hypothesis H that the space is Euclidean and the auxiliary assumption A′ that there is a "differential metrical force field", call it D, of the type specified by SP present throughout the space, will "be compatible with all the data - the data on repeated congruence as well as that which seemed to indicate the non-Euclidean nature of the space" (Sklar, p. 251).*" (Quinn, pp. 394-5).
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> (* indicates where I have changed Quinn's reference in order that it conform to the references in this thesis.)

Both the revised Swanson procedure and Sklar's procedure serve as counter-examples to Grünbaum's 'falsifying' counter-example. Both are legitimate for as Quinn writes: "freedom of stipulation his geometry.[307]

Thus it is apparent that Quinn finds the procedure to be a logically possible manoeuvre, and one which will defend the D-thesis against Grünbaum's attack.

Yet another counter-example was devised by Lawrence Sklar (see footnote 55). We will not discuss this here, since the point against Grünbaum has been made effectively by the previous example, and also since Quinn has thoroughly discussed the Sklar procedure in his paper (see Quinn pp. 394-396). Quinn correctly points out that none of these procedures prove that any or all hypotheses may be preserved by a suitable A'_{NT}. They merely show that for the case brought up by Grünbaum, there are effective procedures for determining an appropriate A'_{NT}. They show that

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in respect to physical space is intrinsically metrically amorphous." (Quinn, p. 395.) Quinn correctly points out that neither of these procedures prove that any or all hypotheses may be preserved by a suitable A'_{NT}. They merely show that for the case brought up by Grünbaum there are effective procedures for determining an appropriate A'_{NT}.

Note: Quinn always refers to D_2, the subthesis of the D-thesis, (see footnote 96) as the "subthesis D_2", this distinguished it from the Swansonian magnitude D_2.

[307] Quinn, ***op. cit***., p. 394.

> "It thus requires the use neither of Swanson's hypothesis that **prima facie** chemically identical rods are really different nor of the hypothesis that **prima facie** chemically different rods are really identical. Hence, RSP is not affected by Grünbaum's warning that to claim chemical difference to be sufficiently theory-laden to save H from a refutation is to introduce an intolerable ambiguity into scientific testing. Indeed, the only restriction on the use of RSP seems to be the following: If one wishes to assert that the differential deformations introduced in A' are the effects of real physical influences rather than a result of a covert stipulation about the lack of self-congruence of differentially unperturbed rods, one must be willing to postulate the existence of physical sources for the forces which cause the deformations. But, from a logical point of view, this can always be done since such existential assertions are by themselves irrefutable. Thus, although the repeated application of RSP may be scientifically otiose, one may always resort to it to satisfy subthesis D_2 with respect to H and any finite set of observations." (***Ibid***.)

Grünbaum has not found a counter-example to the D-thesis. So far, then, we have neither a proof nor a disproof of the D-thesis.

Swanson presented a general procedure for producing an appropriate A'_{NT} for any H, O, and O' (see footnote 22). Quinn examined the objection that "any purely syntactical strategy for generating an A' will necessarily produce a trivial A',"[308] and found that "Swanson could, in any case, propose a K so complex that it would be impossible for us to agree on whether or not it was trivial on the basis of our unaided intuitions."[309] Even so Quinn feels that it is possible "that there may not be a K satisfying all Swanson's conditions."[310] He criticizes Swanson for not demonstrating the consistency of his proposed A'_{NT},[311] and so as matters stand it is possible that Swanson's strategy

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308 ***Ibid***., p. 397.

309 ***Ibid***.

310 ***Ibid***.

311 ***Ibid***., pp. 397-8.

> “It is certainly possible to construct a consistent first order theory K with sole observational consequence O' and all non-observational constants distinct from those of T_0. And likewise, it is possible to construct T'_0 by removing from the axioms of T_0 one or more of the A_i found in the H, $A_{i1}, A_{i2}, \ldots, A_{ik} \vdash O$. What Swanson has not shown is that $T'_0 \sqcup K$ will be consistent for any T'_0 and K constructed in this fashion. This is because there might be more than one proof of 0 in T_0. Even if we have removed from T_0 all the A_i found in the proof H, $A_{i1}, A_{i2}, \ldots, A_{ik} \vdash O$, we have not shown that there is no other proof of O from H and the remaining A_j of T_0. And if there were such a proof, then, since from K one can deduce $O'$and from T'_0 one could deduce O, from $T'_0 \sqcup K$ one could deduce O and O' so that $T'_0 \sqcup K$ would be inconsistent. Moreover, even if one could show that O cannot be deduced from T'_0 alone, it does not follow that O cannot be deduced from $T'_0 \sqcup K$. If $T'_0 \sqcup K$ is not a conservative extension of T'_0, then it is possible to deduce in $T'_0 \sqcup K$ theorems couched in the vocabulary of T'_0 alone which are not deducible in T'_0. But O might be just such a theorem, in which case $T'_0 \sqcup K$ would again be inconsistent. Thus, I conclude that Swanson has not demonstrated, but merely asserted, that there always is a K such that $T'_0 \sqcup K$ is consistent.
>
> Furthermore, even if it were, perchance, to turn out that such a K does exist for some cases, it is clear that Swanson has provided us with no guarantee that $T'_0 \sqcup K$ will satisfy the requirement that H be

could create an inconsistency. Yet Quinn, on the other hand, has not demonstrated that Swanson's strategy must result in an inconsistency. Quinn further criticizes Swanson for not showing that his strategy will result in a system where H is relevant to the deduction of O′.

Quinn's criticisms are telling on Swanson's procedure. Swanson would probably be the first to admit that his procedure was only a sketch to show the possibility of developing a complex A'_{NT} and not a fully "worked-out" theory.

To this point no general proof of the D-thesis has been given, and, on the other hand, there has been no general disproof.

We must conclude that with his special case Grünbaum has not succeeded in establishing a conclusive counter-example.

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substantively relevant to O′. ... 0′ is a theorem of K, but, since all the theoretical predicates and individual constants of K are distinct from those of T_0, H, which is an axiom of T_0 (and, presumably, contains some of the theoretical predicates or individual constants of T_0), will not even be formulable in K and, hence, will not occur in the proof of O′ in K. Therefore, H will not be relevant to the proof of O′ in K, and Swanson has not shown that there is an A′ which can be used in conjunction with H to explain O′. Now it may happen, perchance, in some case that in $T'_0 \sqcup K$ there is another proof of O′ to which H is relevant, but Swanson has given us no reason for supposing this to be so in any particular instance or in general. Therefore I conclude that Swanson has not shown there to be a K which will consistently extend $T_{O'}$ to include a deduction of O′, but not of O, for all T_0, and that, even in cases where such a K exists, he has not provided any reason to suppose that in the resulting $T'_0 \sqcup K$ there will be an A′ such that $(H \& A') \rightarrow 0'$." (***Ibid***.)

Grünbaum's Second Counter-example: **The General Case**

The second part of Grünbaum's counter-example is a critique of "Einstein's Duhemian argument as applied to the empirical determination of the geometry of a region which **is** subject to deforming influences."[312] In this case, since there are deforming influences, laws must be used in making the corrections for the deformations. These laws do presuppose a geometry, for certainly they will involve areas and volumes in order to deal with the elastic stresses and strains. So now, "the empirical determination of a geometry involves this joint assumption of a geometry and of certain collateral hypotheses."[313]

Grünbaum argues that under certain conditions in order to preserve Euclideanism, Einstein may need to abandon the customary definition of the word ‘congruent’ in favour of another.[314] To do so would violate the requirement of semantical stability, Grünbaum's necessary condition for the

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312 Grünbaum, ***Philosophical Problems*** ... ***op. cit***., p. 139.

313 ***Ibid***.

314 ***Ibid***.

" Now suppose we begin with a set of Euclideanly formulated physical laws P_0 in correcting for the distortions induced by perturbations and then use the thus Euclideanly corrected congruence standard for empirically exploring the geometry of space by determining the metric tensor. **The initial stipulational affirmation of the Euclidean geometry G_0 in the physical laws p_0 used to compute the corrections in no way assures that the geometry obtained by the corrected rods will be Euclidean**! If it is non-Euclidean, then the question is: What will be required by Einstein's fitting of the physical laws to preserve Euclideanism and avoid a contradiction of the theoretical system with experience? Will the adjustments in P_0 necessitated by the retention of Euclidean geometry entail merely a change in the dependence of the length assigned to the transported rod on such nonpositional parameters as temperature, pressure, and magnetic field? Or could the putative empirical findings compel that the length of the transported rod be likewise made a nonconstant function of its position and orientation as independent variables in order to square the coincidence findings with the requirement of Euclideanism?". (***Ibid***.)

non-triviality of the D-thesis. Grünbaum knows that Einstein would not wish to abandon the customary definition of 'congruence' since Einstein has criticized Reichenbach for such a procedure.[315]

Grünbaum realizes that he must go further if his argument is to work against the D-theoretic position in general. He writes:

> To be sure Einstein's geometric articulation of that thesis does not leave room for saving it by resorting to a remetrization in the sense of making the length of the rod vary with position or orientation even **after** it has been corrected for idiosyncratic distortions. But why saddle the Duhemian thesis as such with a restriction peculiar to Einstein's particular version of it? And thus why not allow Duhem to save his thesis by countenancing those alterations in the congruence definition which are remetrizations?[316]

My dispute with Grünbaum is over his claim that the D-theorist would accept a restriction which prevents the introduction of an altered congruence definition. Grünbaum claims that:

> ... it is of the essence of Duhem's contention that H (in this case, Euclidean Geometry) can always be preserved not be tampering with the principal semantical rules (interpretive sentences) linking H to the observational base (i.e., the rules specifying a particular congruence class of intervals, etc.), but rather by availing oneself of the alleged **inductive latitude** afforded by the ambiguity of the experimental evidence to do the following: (a) leave

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315 He writes: "Hence in order to retain Euclideanism, it would then be necessary to remetrize the space in the sense of abandoning the customary definition of congruence. ...But this kind of remetrization, though entirely admissible in other contexts, does not provide the requisite support for Einstein's Duhemian thesis: For Einstein offered it as a criticism of Reichenbach's conception." (***Ibid***., p. 142).

316 ***Ibid***., p. 142.

> the factual commitments of H essentially **unaltered** by retaining both the statement of H and the principal semantical rules linking its terms to the observational base, and (b) replace the set A by A′ such that A and A′ are logically incompatible under the hypothesis H. **The qualifying words 'principal' and 'essential' are needed here in order to obviate the possible objection that it may not be logically possible to supplant the auxiliary assumption A by A′ without also changing the actual content of H in some respect**.[317]

I fail to see why such a restriction is "of the essence of Duhem's contention". In fact, I think it is of the essence of the D-theorist's contention that such alterations are permissible in order to save an hypothesis. The thesis that "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" seems to give the D-theorist tremendous latitude for making his changes - that includes semantical 'tampering'. We shall try to show in the next section of this chapter why we feel the restriction of semantical stability is an unnecessary restriction not only on the D-thesis but also on science in general. First, let us continue our examination of Grünbaum's argument.

Some changes constitute violations of semantical stability and some do not. Grünbaum gives an example of a change that does not.

> Suppose, for example, that one were to abandon the optical hypothesis A that light will require equal times to traverse congruent closed paths in an inertial system in favour of some rival hypothesis. Then the semantical linkage of the term "congruent space intervals" to the observational base is changed to the extent that this term no longer denotes intervals traversed by light in equal round-trip times. But such a change in the semantics of the word "congruent" is innocuous in this context, since **it leaves wholly intact the membership of the class of spatial intervals that is referred to as a "congruence class**."[318]

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[317] ***Ibid***., p. 143.

[318] ***Ibid***.

The reason why this is not a violation of semantical stability is that it presumably "leaves intact both the "**principal**" semantical rule governing the terms "congruent" and the "**essential**" factual content of the geometric hypothesis H."[319] Now what Grünbaum needs to specify is just what constitutes a **principal** semantical rule and **essential** factual content. This is done in a later paper which we shall consider in the next section on semantical stability.

There are two ways of changing the essential factual content of an hypothesis. It can be done "either by preserving the original statement of the hypothesis while changing one or more of the principal semantical rules or by keeping all of the semantical rules intact and suitably changing the statement of the hypothesis."[320] Now in the first case while one retains the "**linguistic trappings**"[321] that is, one retains the syntactical or grammatical form of the sentence, one is really changing the factual commitments of the sentence, Thus Grünbaum claims to save a geometric hypothesis by changing the semantical rules for "congruence" is to change the hypothesis. The hypothesis makes a different claim. It has different factual commitments, It is no longer the same statement. He writes:

> That the thus "preserved" Euclidean H actually repudiates the essential factual commitments of the **original** one is clear from the following: the original Euclidean H had asserted that the coincidence behaviour common to all kinds of solid rods is Euclidean, **if** such transported rods are taken as the physical realization of congruent intervals; but the Euclidean H which survived the confrontation with the posited empirical findings only by dint of a **remetrization**

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[319] ***Ibid***.

[320] ***Ibid***.

[321] ***Ibid***., p. 144.

> is predicated on a denial of the very assertion that was made by the original Euclidean H, which it was to "preserve".[322]

Certainly, the claim is different. If it were not different then the new hypothesis would entail the observational consequences that did not obtain. We want an hypothesis that will entail what is observed to obtain, so we have to alter it in one of the two ways suggested above. We either change the sentence or else we change the semantical rules so that the sentence means something different. Which course one chooses depends on a number of factors. These are factors such as simplicity efficacy of communication, efficiency, preferences of working with certain theories (e.g. Euclidean geometry) and so on. Certainly the hypothesis has changed after the alteration of the semantical rules governing "congruence": - that is precisely why the D-theoretician makes the alteration. Some alterations are better than others. In the case of preserving Euclidean geometry, the dispute will be whether or not such preservation does what we want it to more effectively than some other kind of alteration.

Grünbaum tries to disparage the D-thesis by making it look ridiculous. Grünbaum's **reductio**, is an exaggeration and tends only to mislead the reader about the D-thesis by failing to tell the reader the whole story. He writes:

> It is as if a physician were to endeavour to "preserve" an a priori diagnosis that a patient has acute appendicitis in the face of a negative finding (yielded by an exploratory operation) as follows: he would redefine "acute appendicitis" to denote the healthy state of the appendix![323]

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322 ***Ibid***.

323 ***Ibid***.

Unless the physician informed the patient of his change in semantics, he would be lying to his patient and thus not communicating properly with his patient. If the physician informed the patient of his change in semantics, besides wondering about the sanity of his doctor, the patient will know exactly what his condition is by making the appropriate translation from the physician's language to ordinary language. Certainly, the D-thesis permits such trivial changes, but who wants them - not a respectable physician and certainly not a respectable D-theorist. Both want to be able to communicate properly with their fellow man. Both want to or ought to want to do proper science. When someone like Einstein wishes to preserve an Euclidean hypothesis by making an appropriate change in the meaning of "congruence", he does not keep it a secret that he is making such an alteration, he announces it so that the other scientists get the factual commitments of his statement correct. By such an announcement, he frees himself from being saddled with the previous interpretation of the sentence which proved to yield consequences that did not obtain, There is no reason why semantical tampering should be forbidden provided one announces one's changes so that one's audience is aware of the claims being made. Thereafter the community will judge whether this was the most efficient change or whether it might have been better to account for the recalcitrant experience in some other way. There is no need to confine the D-theorist in the way that Grünbaum suggests.

Grünbaum's restriction derives from the logical positivist's habit of treating the statement as the unit of empirical significance, instead of thinking of the unit as the whole system of statements. The D-theorist's view is that the statement derives its meaning from the system of which it is a part. The meaning of a statement remains stable only so long as the system of which it is a part remains stable. If we retain an hypothesis H in the face of disconfirming instance, then what this means is that we have to alter the set of logical consequences of H so that it includes O′ instead of O. We do this by altering some part of the auxiliary assumptions that are used in conjunction with H to deduce O, so that the H in conjunction with the new set of auxiliary assumptions gives O′. H thus has a different set of logical consequences and hence a different meaning. With the change of the appropriate assumptions, the meaning of H changes. One of its logical consequences becomes O′ and one of its logical consequences ceases to be O.

If we change the linguistic form of H while retaining the same semantical rules and the same auxiliary assumptions (- the way we have been using these terms, we have been including the former in the latter), we are rejecting H as an acceptable statement in the system. In this case all that we are doing is deciding that some other area of the system needs to be preserved in the face of the recalcitrant experience and that the hypothesis of the form H is the one that will be sacrificed to bring the system in line with the empirical findings. H will no longer serve as one of the acceptable statements of the system. Basically, the demand for semantic stability is a throwback to the old positivist's notion that the statement is the unit of empirical significance.

If one holds the D-thesis while holding the positivist's notion of empirical significance, one is led into contradiction (as we shall see in the next chapter). So long as one views the unit of empirical significance as the whole of the system, other more pragmatic matters enter into the decision of whether to change the linguistic form of H or to retain it in the face of recalcitrant experience. Certainly retention of such trivial hypotheses as "Ordinary buttermilk is poisonous" or

"Acute appendicitis denotes the healthy state of the appendix" is silly. These would be ruled out by the pragmatic criteria. Still it is logically possible to retain these hypotheses if one wished, but who would wish to do so? A lot of ridiculous things lie in the realm of the logically possible.

Just before we leave our discussion of Grünbaum's counter-example, let us take note of a comment by Robert Barrett in his paper "On the Conclusive Falsification of Scientific Hypotheses,"[324] Barrett picks on Grünbaum's use of the word 'conclusive', In the example of Grünbaum, the hypothesis is not **conclusively falsified** in the **deductive** sense of 'conclusive' that is indicated by the asymmetry thesis, The sense of 'conclusive' that is employed is the much weaker claim of 'with a high degree of probability', Barrett writes:

> ...it is clear that H's falsification is nondeductive, is not irrevocable, and is beset with ordinary inductive uncertainty.[325]

This weakening results from Grünbaum's admission of the theory-ladeness of the certification of the coincidence of two chemically different solids under transport (however, minimal). This admission reduces his claim from a deductive one to an inductive, and hence a fragile, claim.

Barrett says:

> The inductive warrant which inductive evidence can provide for the truth of a given hypothesis is subject to no upper limit short of absolute deductive certainty. If it is fair to say

[324] Robert Barrett, "On the Conclusive Falsification of Scientific Hypotheses" ***Philosophy of Science*** vol. 36 no. 4. (Dec., 1969) pp. 363-374.

[325] ***Ibid***., p. 366.

> Grünbaum's example conclusively falsifies H then it is equally fair to say that scientific hypotheses can be conclusively verified as well.[326]

The ironic thing about this revelation is the realization that "the logic of the example actually puts him squarely on the side of those who argue **for** symmetry between verifiability and falsifiability."[327]

Later, in his Thalheimer Lecture,[328] Grünbaum recognized the import of his earlier claims:

> The irremediable inconclusiveness of the **verification** of an auxiliary component of a total theory by its supporting evidence imposes a corresponding limitation on the deducibility of the categorical **falsity** of the main component of the total theory via other evidence adverse to the total theory.[329]

We will discuss the logic behind this realization in greater detail later in the chapter when we consider Grünbaum's later paper. Barrett's important conclusion is that "Grünbaum's Falsification Example, even if fully sound, not only in no way refutes the symmetry thesis, but actually provides evidence for it."[330]

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326 ***Ibid***.

327 ***Ibid***.

328 A. Grünbaum, "Can we Ascertain the Falsity of a scientific Hypothesis" ***Studium Generale*** vol.22 Fasc. 11 (1969) pp. 1061-1093.

329 ***Ibid***., p. 1067. (See also Barrett, p. 367.)

330 Barrett. ***op. cit***., p.367.

Semantical Stability

Grünbaum has been quite vague about the notion of semantical stability, even though it functioned as the basis of his criticism of the D-thesis. It was in the Thalheimer paper,[331] (May, 1969) where he first became more explicit about his restriction on the D-thesis.

In the early part of this chapter, we discussed Grünbaum's views on what he called the trivial and uninteresting move of saving the statement "Ordinary buttermilk is highly toxic" by changing the rules of English so that the intension of 'ordinary buttermilk became that of 'arsenic'. It was this kind of move which prompted him to make semantical stability a necessary condition for the non-triviality of A′.

Mary Hesse, in her review of Grünbaum's paper "The Falsifiability of a Component of a Theoretical System",[332] wrote:

> ... it is not clear ... that 'semantic stability' **is** always required when a hypothesis is non-trivially saved in face of undermining evidence. The law of conservation of momentum is in a sense saved in relativistic mechanics, and yet the usage of 'mass' is changed - it becomes a function of velocity instead of a constant property. But further argument along these lines is idle without more detailed analysis of what it is for a hypothesis to be the 'same' and what is involved in 'semantic stability'.[333]

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331 Grünbaum, "Can We Ascertain..." (see footnote 2(j)) ***op. cit***.

332 Mary Hesse, Review, ***The British Journal of Philosophy of Science***, vol. 18. (1968) pp. 333-5

333 ***Ibid***., p. 334.

In his attempt to answer Hesse's criticism, we find that Grünbaum appeals to the work of Peter Achinstein in order to specify the notion of 'semantic stability'. In particular, he refers to Achinstein's work on the notion of relevance - that is, what it is for a property to be relevant for an item to be counted as being a certain so and so, say X.

There are two distinctions made: positive and negative relevance, and semantical and nonsemantical relevance. The first distinction is made as follows:

> If the fact that an item has P tends to count more in favour of concluding that it is an X than the fact that it lacks P tends to count against it, P can be said to have more positive than negative relevance,[334]

The second distinction goes as follows:

> Suppose $P_1, \ldots, P_n$ constitutes some set of relevant properties of X. If the properties in this set tend to count in and of themselves to some extent, toward an item's being classifiable as an X, I shall speak of them as **semantically relevant** for X. If the possession of properties by an item tends to count to an X-classification solely because it allows one to infer that the item possesses properties of the former sort, I shall speak of such properties as **nonsemantically relevant** for X.[335]

This distinction becomes more clear with an example. The properties, red metallic element of atomic number 29, good conductor, melts at 1083°C are semantically relevant for copper. Being

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[334] Grünbaum, "Can We Ascertain..." (see footnote 2(j)) ***op. cit.*** , p. 1074.

[335] ***Ibid***., p. 1075.

a substance mined in Ontario is nonsemantically- relevant for copper. We do not classify something as copper because it has such a property.

Achinstein says that he has used "the labels semantical and nonsemantical relevance because X's semantically relevant properties have something to do with the meaning or use of the term 'X' in a way that X's nonsemantically relevant properties do not."[336] Questions relating to semantically relevant properties are questions of meaning. It is important to note that logically necessary semantically relevant properties are negatively semantically relevant properties; - to use Achinstein's formulation, "considering various backgrounds and appropriate weights, the lack of P_i by these substances tends to count in and of itself at least to some extent, against classifying them as X's."[337] Logically sufficient properties are positively semantically relevant, that is "considering various backgrounds and appropriate weights, the possession of Pi by such substances tends to count in and of itself, at least to some extent, in favor of classifying them as X's."[338]

Using Achinstein's distinctions, Grünbaum characterizes a **semantically stable** use of a term 'X' as one in which "no changes are made in the membership of the set of properties which are

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[336] P. Achinstein, ***Concepts of Science*** (Baltimore, 1968), p. 9.

[337] ***Ibid***., p. 15.

[338] ***Ibid***., p. 16.

Note: Not all properties that are semantically relevant (either negatively or positively) are either logically necessary or logically sufficient.

semantically relevant to being an item denoted by the term 'X'."[339] Using this definition, he characterizes the notion of 'the same' used in conjunction with an hypothesis H. He claims that the

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[339] ***Ibid***., p. 1076.

Achinstein's distinctions provide a way of dealing with properties that are not logically necessary or logically sufficient for an item being of a particular sort. "By saying that a property P is logically necessary for being an X I mean that , as the term "x" is used, an item lacking P cannot correctly be classified as an X no matter what other properties it has." (Achinstein, ***Concepts of Science*** (Baltimore, 1968), p. 3.) "When I speak of a property as logically sufficient for being an X, I mean that, as the term "X" is used, an item possessing this property is correctly classifiable as an X no matter what other properties it has." (Achinstein, p.5.)

Achinstein introduces the concept of relevance in order to deal with those properties that are not the above-mentioned sorts. He speaks of a property "as relevant for being an X". (Achinstein, p. 6.) He explains: "By this I mean that if an item is known to possess certain properties and lack others, the fact that the item possesses (or lacks) the property in question normally will count, at least to some extent, in favour of (or against) concluding that it is an X; and if it is known to possess or lack sufficiently many properties of certain sorts, the fact that the item possesses or lacks the property in question may justifiably be held to settle whether it is an X." (Achinstein, p. 6.)

Achinstein considers the example of the term 'metal'. He writes: "The property of high (electrical and thermal) conductivity is relevant though neither logically necessary nor logically sufficient for being a metal. If an item is known to possess certain other properties (for example, hardness and metallic lustre), then the fact that it does have high conductivity will normally count, to some extent, in favor of concluding that it is a metal (though the possession of other properties may count against such a conclusion). And if it is known to possess sufficiently many properties of certain sorts, the fact that it has high conductivity might be taken to settle the matter. If it does not have high conductivity, this does not necessarily preclude it from being a metal; some item might be discovered which lacks this property but has sufficiently many others of appropriate sorts to be classifiable as a metal. However, the fact that an item lacks high conductivity will normally count as some reason against concluding that it is a metal; and if it lacks certain other properties as well, this fact may be taken to settle the question." (Achinstein, pp. 6-7.) He goes on to add: "In classifying properties with respect to relevance, we must, as indicated, consider what would **normally** happen. There might be circumstances in which the fact that an item lacks high conductivity would not count at all against its being a metal ...; but these would be special - for example, if the item were to be heated to an extremely high temperature, thus radically decreasing its electrical conductivity." (Achinstein, p. 7.)

In the light of this, Achinstein makes his distinctions between positive and negative relevance and semantically and non-semantically relevant. The key point to note about the latter distinction is the following: "X's semantically relevant properties have something to do with the meaning or use of the term "X" in a way that X's nonsemantically relevant properties do not." (Achinstein, p. 9.) It is this feature that Grünbaum wishes to exploit.

various terms 'X_i' (i = 1,2,3, ..., n) which constitute the vocabulary of the sentence H which expresses a certain hypothesis are similarly classified as semantically stable. This is so "even though these terms will, of course, not be confined to substance words or to the three major types of terms treated by Achinstein"[340] that is, confined to: (a) physical objects or stuffs, (b) somewhat more abstract concepts applicable to physical objects, stuffs, phenomena, etc. e.g., Bohr atom, (c) quantities capable of numerical degree e,g., velocity.[341] Thus "if the entire sentence H is used in a semantically stable manner, then the hypothesis H has remained the same in face of other changes in the total theory."[342]

These definitions, although they might form the beginnings of a clarification of the notions of semantic stability and of what it is for an hypothesis to be the same, do not take us far enough. As matters stand, Grünbaum has merely postponed clarification until someone has adequately answered the question of what it is for a property to count in and of itself toward an items being classified as a particular kind of thing. The account given in Grünbaum's paper and in Achinstein's book is too vague for anyone to determine the sufficient conditions for picking out semantically relevant properties. All that Achinstein seems to suggest is to "look to what scientists do in actual situations of classification and to what, in fact, they say about such situations; we must also determine what they

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340 Grünbaum, "Can We ..." (2(j)) ***op. cit***., p. 1076.

341 Achinstein, ***op. cit***., p. 2.

342 Grünbaum, "Can We ...", (2(j)) ***op. cit***., p. 1076.

would do and say about hypothetical ones."[343] This is all right so long as the scientists agree, but what do you do in the case of a dispute. It is likely that the cases where the D-thesis will be employed will be such difficult cases that there will be disagreement. It seems as if one is forced into the old essence-accident discussion again. Until Grünbaum or someone else makes clear what it is for a property to count in and of itself toward an item's being classified as a particular thing, then Grünbaum and others cannot restrict the D-theorist to some such thing as semantic stability (for who knows what that would be).

By ruling such semantical changes out of court, Grünbaum is prejudging the issue, I would argue that such moves are logically open to the D-theorist. The question is whether it is worth making such drastic changes. We also have to consider the criteria by which one can determine these questions of worth. In the case of Grünbaum's 'buttermilk' example, the D-theorist would probably

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343 Achinstein, ***op. cit***., p. 8.

Semantic stability based upon Achinstein's notion of semantical relevance will not serve to perform the task that Grünbaum wants it to do. As we indicate in the body of the paper, scientists do disagree. A case where a semantical shift is required to save an hypothesis is exactly where one will find scientists disagreeing. Achinstein, himself, is aware of the inadequacies of his notion. He admits it is an idealization. He writes: "For some properties we may simply not know what to say" (***Concepts of Science***. p. 16) "The regularities governing the use of a term are not so definite that with respect to every relevant property such questions can be answered." (***C. of Sc***. p. 16.) Later:

> "Since theories change, and since we cannot predict how they will change, the question of whether the lack of a theoretical property by some substance counts in and of itself against an X-classification cannot be answered. Even if it now counts, in future years, when the theory changes, it might not." (***C. of Sc***. p. 17)

Achinstein grants that we can give consideration to "linguistic practice at a given time and with respect to current theory." (***C. of Sc***. p. 17). It is precisely in situations such as these, where theory changes, where the D-thesis is applied. These are the cases where we find a lack of semantic stability. Achinstein's semantical relevance will not do for Grünbaum precisely because it is a relative concept, relative to a particular time, and a particular scientific-cultural milieu. This will not do to assure semantical **stability**.

not wish to make such a move and instead would prefer to let the sentence go as falsified. (To make such a change would involve too many changes in other sentences, e.g., we would have to change such sentences as "Ordinary buttermilk leaves a map on the glass after one has consumed the liquid" and besides one would have a great deal of trouble communicating various things about ordinary buttermilk to other native speakers of the English language.) The matter would be very different if the sentence or hypothesis in question was of a more fundamental sort. In such a case it may be quite worthwhile to alter the semantics of certain terms, for example, it may result in a better theory to do so. It might be a neater system. It might be more efficient to do things in the revised way and so on.

In spite of the fact that the D-theorist may not wish to support the hypothesis, H, in the 'buttermilk' situation, the possibility of its support in the described way always remains. If a D-theorist did exercise that possibility, then he would have to suffer the consequences of whatever changes he made in his language to preserve the H. (For example: he may have difficulty communicating with his fellow scientist depending, of course, on how drastic a change he is proposing. Also, he may have to undergo the process of convincing his fellow scientists of the efficacy of such a change.) If one were trying to preserve an Euclidean description of the universe, then semantical tampering with the meaning of terms, such as ‘congruent’, could be justified -that is, if it gives a better scientific description than any other available.

Certain remarks in Grünbaum’s Thalheimer lecture indicate a softening of the stand taken on the D-thesis in his early papers. For example, after he spends eleven pages discussing two historical examples of crucial experiments, "the purported disproof of the occurrence of spontaneous generation of life" and "the claim that R.H. Dicke's observations of the flattening of the sun disprove Einstein's general theory of relativity,"[344] he observes that "the logical situations encountered in our historical examples exhibit the inadequacy of the received textbook account, according to which a component hypothesis can be refuted once and for all.[345] This is a far cry from his original position.

In that paper, Grünbaum adopts Quinn's position that the D-thesis breaks into two subtheses:

($D_{1)}$ - "No constituent hypothesis H of a wider theory can **ever** be sufficiently isolated from some set or other of auxiliary assumptions so as to be separately falsifiable observationally"'[346] and

(D_2) If T is a theory of any domain of empirical knowledge and H is any of its component subhypotheses and A is the collection of the remainder of its subhypotheses and H & A entail the observationally testable consequences O, but in fact O' which is incompatible with O results, then "for all potential empirical findings O' of this kind, there exists at least one suitably revised set of auxiliary assumptions A' such that the conjunction of H with A' can be held true and explains O'."[347]

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[344] Grünbaum, "Can We ..." (2(j)) ***op. cit***., p. 1062.

[345] ***Ibid***., p. 1069.

[346] ***Ibid***., pp. 1070-1.

[347] ***Ibid***., p. 1071.

Quinn's division of the D-thesis into two subtheses derives from some work done by Laurens Laudan. Laudan wrote a paper (Laurens Laudan, "Discussion: Grünbaum on 'The Duhemian Argument'," ***Philosophy of Science*** vol. 32 (1965) pp. 295-99.) where he attacks Grünbaum on an historical and exegetical point. He claimed that Grünbaum "has misconstrued Duhem's views on falsifiability." (Laudan, p.295). He claims that Grünbaum is not attacking Duhem's views so much as he is attacking those who have altered Duhem's position. He says that Duhem's claim is much weaker than the D-thesis that Grünbaum attacks. He informs us that Duhem wished to show chiefly that falsification is as inconclusive as verification and that Duhem did not claim that there was an A'_{NT} that could be found to save every falsified hypothesis H. According to Duhem, Laudan tells us, an hypothesis is not falsified unless someone has proven that it cannot be saved, that is unless someone has shown that no A'_{NT} can be found. Thus on this view the onus of proof is on those claiming the separate falsifiability of the hypothesis H. They must show the impossibility of a set A'_{NT} which can save H in the face of the observational findings O'.

According to Laudan, Duhem also argued that crucial experiments are impossible. No experiment could crucially decide in favour of one hypothesis H_1 over another H_2. A given set of observations can support numerous conflicting theories. As Duhem puts it: "If two different theories represent the same facts with the same degree of approximation, physical method considers them as having absolutely the same validity; it does not have the right to dictate our choice." (Duhem, ***The Aim and Structure of Physical Theory***, p. 288.) As Grünbaum puts it: "Duhem's reason is not that H_1 can always be preserved by a suitable A'_{NT} in the race of any evidence; instead, his reason is that though H_1 might indeed be falsified, we cannot infer the truth of H_2, because there may well be at least one other hypothesis H_3 capable of explaining the phenomena but overlooked by the scientist." (Grünbaum, "The Falsifiability ..." (see footnote 2(h)) ***op. cit.***, p. 282.) Here, then, is no claim about the availability of an appropriate A'_{NT}. Thus when talking about the D-thesis, we must distinguish between the weaker thesis of Duhem, that no hypothesis is separately falsifiable, and the stronger thesis of Quine and others that for every falsified hypothesis there is some A'_{NT} which can be found to save the hypothesis in question.

In his paper, Laudan claims that Grünbaum's refutation of the stronger D-thesis (we question whether there is such a refutation) leaves the weaker Duhemian version untouched. If there were such a refutation, then Laudan is right. Philip Quinn has shown that the weaker thesis, what he calls D_1, is logically independent from the stronger thesis, what he calls D_2. We give his proof below.

Laudan also points out that Grünbaum's counter-example is not appropriate to refute the Duhemian thesis. He writes: "a system of geometry (the 'H' in this example) is not the sort of thing which counts for Duhem as an 'isolated hypothesis'. Duhem ... insisted only that isolated hypotheses - not systems of hypotheses such as geometry or classical mechanics - were non-falsifiable. To say that a **set** of hypotheses has been falsified is no refutation of the D-thesis." (Laudan, p. 299.)

As we mentioned above, Philip Quinn, Grünbaum's student, claims that D_1 and D_2 are independent. His statements of D_1 and D_2 are as follows:

(D_1) "No hypothesis H which is constituent of any scientific theory can ever be sufficiently isolated from some set of auxiliary assumptions or other so as to be separately falsifiable by observations." (Quinn, "The Status of the D-thesis" ***Philosophy of Science*** vol. 36, no.4. (Dec., 1969) p. 398.)

(D_2) "For every hypothesis H, auxiliary assumption A and observational statements O and O' such that (H & A) $\underline{-}$? O, and O' and ~ (O & O'), there is an A' such that H & A' can be held true and H & A' explains O'." (Quinn, p. 398)

Quinn's proof of the logical independence of D_1 and D_2 goes as follows:

D_1 and D_2 are logically independent of one another in the sense that either one may be true while the

Taking into account Quinn's reformulation of the D-thesis, Grünbaum restates his previous three claims. He holds that:

1.There are quite trivial senses in which D_1 and D_2 are uninterestingly true and in which no one would wish to contest them.

2. In its non-trivial form, D_2 has not been demonstrated.

3. D_1 is false, as shown by counter-examples from physical geometry.[348]

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other is false. The proof goes as follows. First, suppose that D_1 is true, i.e., that there is no T_i in T which is falsifiable in isolation. Consider a particular conjunction of two statements in some T such that for some O and O',(T_i & T_j) –? O, and O' and -(O & O'), and suppose that there does not exist, say, a $T_{i'}$ such that $T_{i'}$ & T_j can be held to be true and ($T_{i'}$ & T_j) – O'. In order to guarantee that the supposition of the non-existence of $T_{i'}$ is coherent, the requirement of non-triviality,... may be necessary. In this case D_2 is false. In other words, the truth of D_1 is consistent with the claim that there is some observational statement which cannot be explained at all using a given T_i. Secondly, suppose that D_2 is true, i.e., that for every T_i, T_j O and O' such that (T_i & T_j) – ?O, and O', and -(O & O'), there are a $T_{i'}$ and a $T_{j'}$ such that $T_{i'}$ & T_j and T_i & $T_{j'}$ can each be held to be true, and ($T_{i'}$ & T_j)– ?O', and (T_i & $T_{j'}$) –?O'. Consider the situation in which, say, $T_{i'}$ can be held to be true because although $T_{i'}$ – O" for some O" or other and, hence, is falsifiable in isolation, $T_{i'}$ has either been confirmed by O" or not yet refuted by it (perhaps due to the technical impossibility of making the observations relevant to the testing of 0"). In this case D_1 is false. In other words, the truth of D_2 is consistent with the claim that the revised theoretical statement $T_{i'}$ used to explain some O' is itself separately falsifiable and hence, a counter-example to D_1." (p. 386) For obvious reasons, we cannot accept his proof, however, we do believe that the two sub-theses are independent.

348 ***Ibid***., p. 1071.

The first claim is essentially unchanged. With respect to his ‘buttermilk’ example he adds that:

> ... if one does countenance such **unbridled** semantical instability of some of the theoretical language in which H is stated, then one can indeed thereby uphold D_1 in the form of Quine's epigram: "Any statement can be held true come may, if we make dramatic enough adjustments elsewhere in the system." But, in that case, D_1 turns into a thoroughly unenlightening truism.[349]

He continues to rule a "buttermilk" change out of court. He still does this by insisting on non-trivial changes and labelling this kind of change as a trivial one. My feeling, as I have indicated earlier, is that Grünbaum is prejudging the issue by ruling this possibility out of court. The D-theorist would certainly wish to retain this kind of move. The question whether he would wish to make the particular move suggested by the buttermilk example is another matter - to be decided by various pragmatic criteria (which we shall consider later). As indicated earlier, the D-theorist would probably not try to save the "buttermilk" sentence since the pragmatic costs would be too high, but he, unlike Grünbaum, would not argue against the possibility of making the kind of move envisaged by the "buttermilk" example.

In the Thalheimer lecture, Grünbaum tries to refute Mary Hesse’s claim that "the law of conservation of momentum is in a sense saved in relativistic mechanics"[350] by a change in the usage

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[349] ***Ibid***., p. 1072.

[350] Hesse, ***op. cit***., p. 334.

of the term "mass". (".,. it becomes a function of velocity instead of a constant property."[351] If Hesse's claim is correct then it serves as a counter-example to Grünbaum's claim that "semantic stability is a necessary condition for the non-trivial fulfilment of D_1."[352]

Since Grünbaum claims that semantic stability is a necessary condition of the non-triviality of a change, then this says that a change is non-trivial only if the change is a semantically stable one. That is, if Hesse can show that there is one non-trivial change that is not semantically stable, she will have shown that semantic stability is not a necessary condition of non-triviality. This is what Hesse's counter-example is designed to show. Her strategy is to present a theoretical change that Grünbaum would have to agree is non-trivial and then show that it violates the condition of semantic stability. I believe Hesse succeeds.

Clearly Hesse has selected an example that involves a non-trivial change that Grünbaum agrees is non-trivial. Grünbaum admits that it is both "non-trivial and useful for mechanics to use the word 'mass' in the case of both Newtonian and relativistic mass."[353] It is true that as velocities

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351 ***Ibid***.

352 Grünbaum, "Can We..." (2(j)) ***op. cit***., p. 1077.

353 ***Ibid***.

"One postulational base of the special relativistic dynamics of particles combines **formal homologues** of the two principles of the conservation of mass and momentum with the kinematical Lorentz transformations. It is then shown that it is possible to satisfy the two formal conservation principles such that they are Lorentz-covariant, only on the assumption that the mass of a particle depends on its velocity. And the exact form of that velocity-dependence is derived via the requirement that the conservation laws go over into the classical laws for moderate velocities, i.e., that the relativistic mass **m** assume the value of the Newtonian mass $\mathbf{m_0}$ for vanishing velocity. This latter fact certainly makes it non-trivial and useful for **mechanics** to use the word 'mass' in the case approach zero the values for relativistic mechanics go over into the values for Newtonian mechanics. This is something that every freshman in physics is taught. However, this does not mean that the term ‘mass’ as it is used in Newtonian mechanics is synonymous with the term ‘mass’ as it is used in relativistic mechanics, In the latter case it is a function of the velocity and in the former case it is a constant. So it is a reinterpretation of the term ‘mass’ to move from its use in Newtonian mechanics to its use in relativistic mechanics.

Grünbaum writes:

> Using Achinstein’s concept of mere **relevance**, we can say that the Newtonian and relativity theories disagree here as to the membership of the set of properties relevant to ‘mass’. For it is clear that, in Newton's theory, velocity independence is **positively relevant**, in Achinstein's sense, to the term ‘mass’. And it is likewise clear that velocity-dependence is similarly **positively relevant** in special relativistic dynamics.[354]

Thus he admits the positive relevance of velocity-independence and velocity dependence for the term ‘mass’ in each of the two theories. It is semantical relevance that is in question here, for we are concerned to know whether there is a change of meaning. If Hesse’s counter-example is to work then the change from the one use of ‘mass’ to the other use of ‘mass’ should involve a change of meaning. It is at this point that Grünbaum begs the question on Hesse.

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of both Newtonian and relativistic mass. And hence Einstein's **formal** retention of the conservation principles is certainly **not** an instance of unbridled reinterpretation of them. Yet despite the interesting common feature of the term 'mass', and the formal homology of the conservation principles, the two theories disagree here." (***Ibid***.)

[354] ***Ibid***.

He considers the two cases: (i) that the properties are semantically relevant, and (ii) that the properties are semantically relevant. In the first case he begs the question, In such a case, a case where the properties are semantically relevant, the change involves a change in the meaning of the word 'mass'. If our case is one of those, then Hesse would have a counter-example. In this case "Newton's theory can be said to assert the conservation principles in an interpretation which assigns to the abstract word 'mass' the value $\mathbf{m_0}$ as its denotation, whereas relativity theory rejects these principles as generally false in that interpretation."[355] Clearly, then, we have a change in the meaning of the word 'mass', Grünbaum begs the question when he writes: "But a non-trivial fulfilment of D_1 here would have required the retention of the hypothesis of conservation of momentum in an interpretation which preserves **all** of the properties semantically relevant to 'mass' and to 'velocity' for that matter,"[356] Now that is certainly the case if one accepts his necessary condition for non-triviality. **But** that is exactly what is in dispute here. Mary Hesse has denied that semantic stability is a necessary condition precisely because of the difference in interpretation illustrated by this example. So Grünbaum blatantly begs the question when he adds: "Since this requirement is not met" (namely the necessary condition to which Hesse's counter-example is directed) "on this construal, the **formal** relativistic retention of this conservation principle cannot qualify as a non-trivial fulfilment of D_1."[357]

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355 ***Ibid***., p. 1078.

356 ***Ibid***.

357 ***Ibid***. Grünbaum even unwittingly takes his conclusion back in the next sentence where he admits that it is a "semantical reinterpretation". He writes: "But the success of Einstein's particular semantical reinterpretation is, of course, highly illuminating in other respects." (***Ibid***.) He does not specify in what other respects.

In the second case, the case where the properties are non-semantically relevant, Grünbaum correctly points out that in this case Hesse would not have a counter-example. In such a case there would not be a violation of semantic stability. My claim is that Hesse's counter-example is not such a case. Clearly the meaning and use of the word 'mass' in special relativistic dynamics is different from its meaning and use in Newtonian dynamics, This can be shown as follows: Mass is the numerical measure of inertia or that property of a body that resists being linearly accelerated. Under the Newtonian view mass is a scalar quantity and is considered a constant - that is because it is assumed that inertia is an invariant property of a body independent of the position and speed of the body. However, Einstein has shown in his special theory of relativity that the mass of a body is actually a function of its speed, the relationship being the following:

$$m = \frac{m_0}{\sqrt{1 - v^2/c^2}}$$

where, m_0 = the mass of the body when at rest

v = the speed of the body

c = the speed of light

Thus, under the special theory of relativity, there is clearly a difference in the meaning and use of the word 'mass' from the meaning and use in Newtonian mechanics. The mass of a body is no longer considered to be a constant, but rather the equation states that the mass of a body increases as its speed increases, becoming very large when v approaches c. Since the increase in **m** does not become appreciable until v approaches c, so that for ordinary bodies moving with speed small compared with c, the increase in m is negligible and undetectable, we can rely on the Newtonian view of mass for ordinary purposes. As v approaches zero the relativistic mass of a body approaches the value for the Newtonian mass of the body. Hence it was all right for Einstein to retain the word 'mass' under his relativistic interpretation rather than inventing some new word. Clearly, then the reinterpretation of 'mass' in special relativistic dynamics is a change in the meaning of the word 'mass' and clearly this is a case where there is a violation semantic stability in a non-trivial change. Thus Hesse's counter-example holds.

Since Hesse's counter-example is telling, we have to conclude that semantic stability is not a necessary condition of non-triviality. Since Grünbaum does not even attempt to list any sufficient conditions, we will not consider any.

The Impossibility of Grünbaum's Counter-Example:

If we consider what Grünbaum was attempting to do when presenting his counter-example to the D-thesis, we quickly discover that the project cannot go through, given that Grünbaum accepts the asymmetry thesis, that verification is inconclusive whereas falsification is conclusive. Grünbaum's strategy was to show that a case could arise where every non-trivial A' that is capable of preserving the hypothesis H in the face of observational findings O' must be false. If that could happen, then the falsity of the predicted O could only be attributed to a false H, and we would have a case where H could be separately falsified. In order to show this, he needs to show that every true

A'_{NT} that is conjoined to H yields observational consequences that are incompatible with the observed O'. The problem arises in the determination of the true A'_{NT}. How can one determine the truth of the A'_{NT}, if verification is conclusive?

If Grünbaum wants a logically tight counter-example, then he has to allow that verification is conclusive. In such a case, the asymmetry thesis has to be dropped, since both verification and falsification will now be conclusive. However, I am sure that Grünbaum, like most other philosophers, would not wish to hold that verification is conclusive. So now, if verification is considered to be inconclusive, the asymmetry thesis must go, for one cannot show (logically) that an hypothesis is separately falsifiable.

Criticisms similar to the above are found at various places in the literature. One such criticism is found in a paper by Gary Wedeking.[358] He writes:

> ... the ordinary model of the falsification of a hypothesis
>
> $((H \rightarrow O) \cdot {\sim}O) \rightarrow {\sim}H$
>
> of inductive logic is overly simple, Such a model would represent the testing of a physical hypothesis only if we knew that the other assumptions upon which the prediction was based were true. But inductive logic itself assures us that this is something we cannot know, for it is an accepted inductive maxim that a law (or universally quantified statement) can never be completely verified. An adequate logical model for physical experiments must, then, include in the antecedent not only the conditional, of which the consequent is O, not only the

[358] Gary Wedeking "Duhem, Quine and Grünbaum on Falsification", ***Philosophy of Science***. XXXVI, no. 4. (Dec., 1969), pp. 375-380.

> hypothesis, but also these other assumptions A. If the result of an experiment is negative, we cannot, therefore, logically conclude that H is false.[359]

On this view, then, Grünbaum cannot claim that the situation is characterized by the schema

$((H \cdot A) \rightarrow O) \cdot \sim O \cdot A) \rightarrow \sim H$, for the "A can never be inductively certain."[360]

Philip Quinn, also, makes use of this fact in order to show the impossibility of Grünbaum's counter-example. Quinn points out that if we are going to be able to deduce ~H from the schema

$((H \,\&\, A) \rightarrow O) \,\&\, \sim O \,\&\, A) \rightarrow \sim H$ we have to be able to assert each of the conjuncts in the antecedent.

> However, to suppose that we are completely justified in asserting A, i.e. that there are no differential deforming forces present, on the strength of observations that all tested solid rods of different chemical constitution which coincide at one place in R also coincide elsewhere in R is just to suppose that a theoretical statement can be conclusively verified by observations.[361]

Quinn points out that Grünbaum did avoid making this mistake, but that in doing so he left himself open to attack by the D-theorist. Quinn writes:

> Grünbaum, of course, carefully avoided this mistake when he wrote that "A is only more or less highly confirmed by the ubiquitous coincidence of chemically different kinds of solid rods", but he does not seem to have appreciated the consequences of this admission. For as

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359 ***Ibid.***, pp. 375-376.

360 ***Ibid.***, p. 376.

361 Quinn, ***op. cit.***, p. 390.

> long as A is only more or less highly confirmed, the defender of the D-Thesis can deny A in order to try to save H.[362]

Along with Quinn, we can conclude that Grünbaum's counter-example does not show the falsity of sub-thesis D_1 of the D-thesis. All that this shows is that the falsity can be blamed on the auxiliary assumptions A rather than on H, but it does not show that one can find an A'_{NT} to save the H. Neither does it show that such an A'_{NT} cannot be found. The D-thesis has neither been proven or disproven. I rather suspect that it is not the sort of thesis that is subject to proof or disproof without some kind of question-begging taking place. I suspect it is one of those basic methodological theses which guides one's approach to scientific questions. How one decides the question of the D-thesis will determine one's way of viewing the world and one's ability to deal with the world using the modeling techniques of modern science. It is one of those fundamental theses that form one's approach to the world.

Summary:

In this chapter, we have shown that the D-thesis can be successfully defended from the criticisms raised by Grünbaum. By no means have we covered every response, but we have covered a sufficient number of responses to be able to proceed confident that the D-thesis is still an open question. Also, our discussion has opened up a number of issues that will have to be taken up in the later parts of the dissertation. Let us briefly review what has been accomplished in this chapter.

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362 ***Ibid***.

We began with a presentation of the asymmetry thesis and the Duhemian criticism of that thesis. Next we considered Grünbaum's objections to the Duhemian position. We agreed with Grünbaum that there are certain trivial applications of the D-thesis, however we differed with respect to the question of semantic stability. Most of the trivial cases can be avoided by insisting on a sufficiently strong entailment relation. Grünbaum insists that a necessary condition for the non-triviality of a particular change is semantic stability. We feel that such a demand discounts a significant part of the D-thesis, and also results in begging the question. Later in the chapter we used a criticism that originated with Mary Hesse that semantic stability is not a necessary condition for non-triviality.

We considered Grünbaum's counter-example in both its special and its general cases. In both cases we were able to conclude that he had failed to come up with a counter-example which falsified the D-thesis. In the first case, he must either give up his assymetry thesis or else leave room for the D-theorist to make his alternative adjustments by admitting that the observational certification of freedom from deforming influences is theory-laden. We then considered some attempts to show how a D-theorist might devise an appropriate revision to save the geometrical hypothesis in Grünbaum's counter-example (the special case). In the general case of Grünbaum's counter-example, he relied on the notion of semantical stability. In that case we concluded that he was illegitimately blocking the D-theorist's route of escape.

In the next section, on semantical stability, we considered his reactions to Mary Hesse's telling criticisms. We found his explanation lacking (especially with to his reliance on Achinstein's notion of semantic relevance. That notion remains to be filled out.) In fact, we found that Grünbaum softened his position on the D-thesis in his later work, the Thalheimer lecture. Towards the end of that paper he writes:

> I maintain, therefore, that there are cases in which we can ascertain the falsity of a component hypothesis to all scientific intents and purposes, although we cannot falsify H in these cases beyond any and all possibility of subsequent rehabilitation.[363]

This not far from the position of the D-theorist. The D-theorist would certainly grant that there are certain hypotheses which we let go as falsified for all scientific intents and purposes. We may find it simpler to do so or else we find certain of the supporting assumptions to be more valuable and prefer to preserve those instead of the hypothesis in question. All that the D-theorist is claiming is that no individual hypothesis is conclusively falsifiable, and this is what Grünbaum now seems to grant in the latter part of the above quotation. In fact, he goes so far as to grant Hesse's point:

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[363] Grünbaum, "Can We ..." (2(j)) ***op. cit***., p. 1092.

He was led to soften his position by the realization that the verification of A must suffer from inductive uncertainty. His reasoning goes as follows:

"Duhem attributed the inconclusiveness of the falsification of a component hypothesis H to the legitimacy of denying instead any of the collateral hypotheses A which enter into any test of H. Our analysis has shown that **the denial of A is legitimate precisely to the extent that its VERIFICATION suffers from inductive uncertainty**. Moreover, in each of our examples of attempted falsification, the inconclusiveness is attributable **entirely** to the inductive uncertainty besetting the following two **verifications**: the verification of A, and the verification of the socalled observation statement which entails the **falsity** of the conjunction H . A. In short, the inconclusiveness of the falsification of a component H derives wholly from the inconclusiveness of verification. And the falsification of H itself is inconclusive or revocable in the sense that the falsity of H is **not** a **deductive** consequence of premisses **all** of which can be known to be true with certainty." (pp. 1091-2)

> Hence if the falsification of H denied by Duhem's D_1 is construed as **irrevocable**, then I agree with Mary Hesse that my geometry example does not qualify as a counterexample to D_1.[364]

Thus far, then, the D-thesis remains an open question. It has neither been proven or disproven. Philip Quinn has neatly summarized the status of the D-thesis to this point and we shall end the chapter with this summary.

> However, neither D_1 nor D_2 has been proven in full generality. And, since D_1 and D_2 are logically independent of one another, a proof of one would not suffice to establish the other. Furthermore the particular devices which suffice to establish D_2 for the geometrical example do not seem to promise any fruitful generalizations, relying as they do on features of physical geometry which are not known to have counterparts in other domains. On the other hand, Swanson's attempt to provide a perfectly general procedure for satisfying D_2, although it proceeds in the right direction, is demonstrably inadequate.
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> What, then, is the status of the D-thesis? It has neither been proven nor refuted and remains, therefore, to challenge the ingenuity of philosophers of science.[365]

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364 ***Ibid***.

He continues: "But I continue to claim that it does so qualify if one requires only falsification to all intents and purposes of the scientific enterprise."

365 Quinn, ***op. cit***., p. 399.

CHAPTER FOUR

Teasing Out the D-thesis

(D-thesis and Meaning Change)

It should be apparent to the reader by now that the D-thesis has an important semantical application.[366] It advocates meaning change as a means of preserving a particular hypothesis. When one considers Quine's program in the "Two Dogmas" article, it becomes apparent that one can interpret the D-thesis as permitting the restructuring of one's system of science to accommodate one's choice of analytic statements. On this interpretation, analytic statements become those that are held true come what may. They are the statements for which one makes drastic changes in one's system in the case of recalcitrant experience. Since no statement is immune from revision they are the ones given up last. As such they form the basic statements of the system and in a sense they determine the meaning relationships, the logical connections, within the system. What the D-thesis says is that one can hold a given statement to be true in the face of disconfirming evidence by making the appropriate alterations elsewhere in one's system. This could mean altering sentences in the system that determine the meaning relationships or the logical connections. The choice of whether to let the hypothesis go as falsified or to 'preserve' it by making some meaning change is made on the basis of a number of criteria. Ultimately it is a value question - a question of which statement(s) are valued the most.

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366 Not only is this, perhaps, the most significant aspect of the D-thesis, it is not trivial. In fact, I suspect that any non-trivial change, in Grünbaum's sense of non-trivial, will be trivial in the sense that it will be a mere syntactical move and hence logically trivial.

It is clear that Quine intended his form of the D-thesis to encompass changes in meaning. Otherwise, his theory would become entangled with contradiction.

It is apparent that Grünbaum and Quine are at cross-purposes on the question of the D-thesis. Although each would probably not acknowledge that there are different levels of science, we contend that each is talking about a different level of science. The D-thesis applies at both levels, but what determines the level is how the D-thesis is applied or what is changed in response to recalcitrant experience. At the lower level, semantic stability is prized. This is the level where falsification is considered to be conclusive (at least for purposes of working at this level) and crucial experiments are carried out (-again the experiments are only crucial for this limited level of science). One can borrow a phrase from Kuhn and call this level 'normal science'. Here the background is always preserved and the hypothesis is sacrificed. Semantic stability is considered a necessary requirement for working at this level. There is a higher level of science where such procedures are not considered to bc conclusive, and it is at this level where the D-thesis has its most significant application. We can call this the level of 'extraordinary science' as opposed to normal science.[367]

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[367] See T.S. Kuhn, ***The Structure of Scientific Revolutions*** 2nd ed. enlarged, (Chicago, 1970).

I think it is fair to use Kuhn's terminology here, particularly, since it is suited to the matters being discussed, but, also, since Kuhn was influenced by the views expressed in "Two Dogmas". In his preface (p. vi) Kuhn acknowledges this influence.

Kuhn talks of normal science, paradigms, and paradigm changes. He may not wish to talk about levels of science the way we do in this dissertation. Kuhn talks about 'revolutionary science', whereas I prefer to use the phrase 'extraordinary science'.

At the level of normal science, the classical form of falsification does operate. Hypotheses are tested in situations of theoretical stability, that is, where the background theory is kept constant. The researchers in normal science "are committed to the same rules and standards for scientific practice."[368] In this context semantic stability is insisted upon. The meaning relationships set by the theoretical context are retained so that the hypothesis is being tested for a particular set of theoretical assumptions. Thus progress can be made within normal science (i.e., for a particular paradigm of science) through the trial and error technique.

Grünbaum and others in their criticism of the D-thesis seem to have ignored the second level or science, the level of paradigm change. At this level it would be wrong to insist upon semantic stability, for meaning change is precisely the issue. At such a level, paradigm shift or the alteration of meaning relationships are of the essence. A given hypothesis that appears to have been falsified at the level of normal science may be essential to the new theoretical set (paradigm) and hence in need of 'preserving'.[369] The D-thesis operates here to permit such a 'preservation' by altering the auxiliary assumptions (the background theory). To insist on semantic stability at this level would be to beg the question in favour of the established paradigm.

If we are going to tease out the D-thesis, then it might be helpful to return to Quine and to see what more he has to say. Unfortunately, there is no place in Quine's published work where he

[368] ***Ibid***., p. 11.

[369] I have placed "preserve" in single quotations to indicate that although the linguistic form of the statement is being preserved, it may be the case that the factual claims of the statement will have changed as a result of changes in various meaning relationships that result from a paradigm shift.

responds to the criticisms of Grünbaum. He has, however, responded to criticisms put by Professor Harry Frankfurt. Frankfurt's criticisms are more general than those of Grünbaum. Even so, some of his criticisms are directed to points similar to those discussed by Grünbaum. Other of his criticisms are directed to points that Grünbaum had not considered. We will look briefly at Quine's development of the D-thesis in ***Word and Object*** for it is there that he develops the notions that he can use in response to some of Frankfurt's criticisms.

In his paper entitled "Meaning, Truth, and Pragmatism",[370] Frankfurt takes Quine to task for not drawing out the full import of the suggestion that "the unit of empirical significance is the whole of science."[371] Although Frankfurt realizes that Quine is suggesting that the meaning of the empirical statements in science are in some way dependent "on the system of beliefs that forms its context,"[372] he complains that Quine "never explains the exact nature of the relation between the meaning or truth of an empirical statement and the context provided for it by the system of science in which it appears."[373]

Frankfurt is right. If Quine's suggestion is to be of any use, then he needs to spell out the exact nature of the relationship.

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370 Harry G. Frankfurt, "Meaning, Truth and Pragmatism", ***The Philosophical Quarterly*** X (1960) pp. 171-6.

371 Quine, "Two Dogmas..." ***op. cit***., p. 42.

372 Frankfurt, ***op. cit***., p. 172.

373 ***Ibid***., (see footnote 4 on p. 172.)

Frankfurt has extracted from Quine's suggestion a theory about **empirical** truth and a theory about **empirical** meaning. The first is "the theory that an **empirical** statement derives it truth-value from its relation to a system of beliefs."[374] The theory of meaning asserts that 'the meaning of a **synthetic** statement is dependent on the context provided by the system to which the statement belongs."[375] Given this view, Frankfurt claims: "Experience can be accommodated by any one of a number of different systems of beliefs, and any given statement will belong to some of these alternative systems and will be excluded from others."[376] He is alarmed at the openness of this suggestion and questions whether Quine's theory can be construed as an empiricist theory.

> It is difficult to understand on what grounds Quine considers his doctrine entitled to be regarded as a form of empiricism. For it seems to be his view that experience neither compels the acceptance of any belief nor forbids the acceptance of any belief. The demand that our system of beliefs must account for the data of experience is not peculiarly characteristic of empiricism; all philosophers insist that our beliefs are inadequate unless they "save the appearances". But apart from his acceptance of this minimal requirement, Quine seems to leave the connection between experience and rational belief rather tenuous, to say the least.[377]

Naturally, Quine does not consider his theory as leaving the connection between experience and rational belief to be as tenuous as Frankfurt would like to make it out to be. In a letter to

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374 ***Ibid***.

375 ***Ibid***. However, as Frankfurt points out, because Quine rejects the analytic-synthetic dogma, he, thus, "widens the scopes of these theories by removing from them the restrictions implied by such terms as 'empirical', 'synthetic' and 'about the external world', (Frankfurt, p. 172.) The point that Frankfurt is making is that Quine's views about analyticity "results in the extension of his theories about empirical meaning and empirical truth into theories generally applicable to all statements." (Frankfurt, p. 172.)

376 ***Ibid***., p. 174.

377 ***Ibid***.

Frankfurt, He replied that "the empirical content is provided by the stimulus meaning, which is overwhelming in observation sentences and not inconsiderable in other sentences."[378] It is clear from

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[378] Quine - in a letter to Frankfurt dated October 2, 1962. A copy of this letter was provided by my advisor, Professor J.S. Minas. Since this letter is not readily available, I have copied it below.(See also Exhibit I).

Emerson Hall,
Harvard University,
Cambridge 38, Massachusetts
October 2, 1962

Professor Harry Frankfurt,
Department of Philosophy,
The Ohio State University,
Columbus 10, Ohio

Dear Professor Frankfurt:

I have just finished a book on set theory, and have turned at last to clearing up a distressing interim accumulation of neglected reprints and mimeograms. I am shocked now to find among them something older than the rest, and an original typescript at that. It is your "Meaning, truth, and pragmatism" of March 1960. True, you said I could keep it; otherwise I'd have acted somewhat promptly; still I had not meant to delay thus indefinitely.

Ironically, my ***Word and Object*** came out about simultaneously: March 1960. As you may meanwhile have noticed, this book is largely concerned with expanding, supplementing, and improving the doctrine that was so inadequately sketched in those last four pages of "Two Dogmas".

A central point of your criticism was that I leave myself no remnant of empiricism. In terms of ***Word and Object***, Chapter 11, my answer is that the empirical component is provided by the stimulus meaning, which is overwhelming in observation sentences and not inconsiderable in many other sentences. This doctrine is the filling in of what was so briefly and metaphorically hinted in terms of "distance from periphery" in "Two dogmas".

Also, what certainly is vital, there are in ***Word and Object*** disavowals of a too monolithic holism; e.g., p. 13n. Critics of those last pages of Two Dogmas" on this score are certainly not to be blamed. On the other hand the holism still seems right to me in essential respects, and it is what makes for the "indeterminacy of translation" urged in ***Word and Object*** [and foreshadowed in ***From a Logical Point of View***, Essay 3].

Another difficulty that you raised was that since I referred all meaning to the whole system, I retained no way of making sense of sameness of statements. In this connection I would clarify first a minor point of terminology: in my own writings, early and late, 'statement' has always referred to linguistic forms and not this that Quine employs the notion of stimulus meaning to ground his theory in experience. It is interesting to note that he considers the doctrine of stimulus meaning to be "the filling in of what was so briefly and metaphorically hinted in terms of "distance from periphery" in "Two Dogmas.""[379]

The notion of stimulus meaning is developed in ***Word and Object***. Quine wrote that ***Word and Object*** "... is largely concerned with expanding, supplementing and improving the doctrine that was so inadequately sketched in those last four pages of "Two Dogmas"."[380] So Quine considers ***Word and Object*** to be a development of his field theory of knowledge, his D-theoretic position.

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to their meanings. This may have thrown you off in specific passages, but your general point remains important, and it is one that I recognize and discuss in ***Word and Object***, top p. 24 and elsewhere.

I am sorry you did not then have before you a statement of my developing views that was more worthy of your mettle. This mettle I find formidable, and I do not flatter myself that ***Word and Object*** is proof against it; but still I should there expect somewhat less the impression of a steel trap on a butter ball.

And I am doubly sorry for the oversight that has delayed this letter a couple of years longer than mere preoccupation could have done.

Sincerely yours,

W.V. Quine
WVQ/dlr Professor of Philosophy

[379] See letter in footnote 13.

[380] See Quine's letter in footnote 13. Quine's statement is slightly exaggerated. While a good deal of ***Word and Object*** is a development of the field theory of knowledge, a much larger portion of the book is spent in developing logical notions that are only indirectly related to the field theory of knowledge. These latter portions would be better described as largely concerned with expanding, supplementing, and improving Quine's theory of quantification which had been sketched in his early writings.

For a more detailed exposition of Quine's development of the notion the reader is directed to the Appendix Two, entitled "Quine's Development of the Doctrine of Stimulus Meaning". Stimulus meaning is a device "for exploring the fabric of interlocking sentences, a sentence at a time."[381] He employs it as a way of 'getting at' the "net empirical import of various single sentences without regard to the containing theory."[382] This notion, however, only partially resolves the predicament created by trying to talk about the meaning of sentences in the situation where the unit of empirical significance is taken to be the whole theory. A certain amount of relativity remains because in order for an individual to understand the sentence that is being queried, he must have had access to previous stimulation and other collateral information. In spite of this drawback, the device of stimulus meaning does help to unravel the fabric of the language of science, for it enables us to distinguish between the degrees of observationality of certain sentences.

In ***Word and Object***, Quine attempts the beginnings of a classification of sentences according to their observationality. (See Appendix Two). Observation sentences, or those sentences on which there is a great deal of **intra-subjective agreement**, provide the empirical footings of Quine's theory. These sentences are connected in a complex way to the other sentences of the language.
The structure of language, that is, the interrelationships among the sentences of a language, is not easily ferreted out. Nevertheless, Quine attempts a beginning in ***Word and Object***. In one's attempt to get at the internal structure, difficulties arise over the differences in the personal histories of the individual speakers studied. Vague connections with the individual speaker's past stimulations tend

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381 W.V.0. Quine, ***Word and Object*** (Cambridge, 1960) p. 35.

382 ***Ibid***., pp. 34-5.

to interfere. For example, one individual, a physicist, may assent to a sentence where most other individuals usually dissent. These peculiarities of personal histories get in the way of discovering the sentence to sentence connections within a given language. In order to overcome this Quine devises the notion of stimulus synonymy. (For an exposition of this notion see also Appendix Two). Even this notion has its difficulties. Access of the subjects to collateral information results in its relativization. In an attempt to get around this, Quine socializes the concept, and counts only those terms, for example, that "come out stimulus-synonymous for each individual speaker almost without exception."[383] This is a rather limiting constraint to impose on a study that aims at ferreting out the sentence to sentence connections of a language in its complexity.

In spite of the inherent difficulties, Quine briefly discusses the role of terms and logical connectives within his program. Both logical connectives and certain terms like 'bachelor' and 'unmarried man' are learned from sentential contexts rather than by some form of ostension. Detached from their semantical involvements in these contexts, one has very little to deal with, for their meaning seems to be entirely derivative or nearly so. Both are in some respect part of our own special apparatus of objective reference - part of our peculiar way of conceptualizing and this makes it difficult to pin them down by means of behavioural criteria.

From the point of view of developing the D-thesis, ***Word and Object*** is disappointing. Certainly, Quine has indicated a means of grounding his theory in experience through the notion of stimulus meaning, but this remains only an indication so long as he leaves us hanging on the question

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[383] ***Ibid***., p. 55.

of the basic weave pattern of the fabric of language or on how to discover it. He had hoped to find in ***Word and Object*** some indication of how sentences other than occasion sentences were related to experience. Not all sentences are directly related to experience. Quine tells us that the significant trait of these other sentences "is that experience is relevant to them largely in indirect ways, through the mediation of associated sentences."[384] What we are looking for is some account of how this mediation through associated sentences works. Quine does not tell us in ***Word and Object***, he merely reverts to a restatement of the D-thesis. He writes:

> Alternatives emerge: experiences call for changing a theory, but do not indicate just where and how. Any of various systematic changes can accommodate the recalcitrant datum, and all the sentences affected by any of those possible alternative readjustments would evidently have to count as disconfirmed by that datum indiscriminately or not at all. Yet the sentences can be quite unlike with respect to content, intuitively speaking, or role in the containing theory.[385]

What Quine is saying is that there are many different ways of seeing the sentence to sentence connections of a given language. Each of these ways differ, for example, in the way that one would alter a theory to take account of a recalcitrant experience. To leave matters as vague as this will not do. Frankfurt's complaint that Quine "never explains the exact nature of the relation between the meaning or truth of an empirical statement and the context provided for it by the system of science in which it appears"[386] is well taken. Quine leaves the matter hanging even in ***Word and Object***.

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[384] ***Ibid***., p. 64.

[385] ***Ibid***.

[386] Frankfurt, ***op. cit***., p. 172 (see footnote 4 on that page).

Quine disappoints us with respect to filling out the metaphor of "Two Dogmas...". Certainly, he has shown us how he intends language to be grounded in experience through his account of stimulus meaning . Also he does begin to classify sentences according to their empirical content, but he does not take this analysis far enough to reveal the fabric of language. We learn that **occasion sentences** are the most empirical since there is a direct correspondence between stimulation and an individual's response to these sentences. (**Observation sentences** are a kind of occasion sentence.) However, even these sentences are not purely empirical, since they suffer the effects of prior stimulation and collateral information. **Standing sentences** are less empirical since their meaning depends more on their relationships within language. Their connection to the world is less direct or more indirect than occasion sentences. Quine tells us that this connection is made through the medium of associated sentences. This is where Quine lets us down, because he does not fill out the relationship involved here. The remainder of ***Word and Object*** is no help either since it is basically an elaboration of Quine's theory of reference and a continuation of his search for an austere canonical form for the system of the world.

Quine's theory clearly has an empirical basis, observation sentences provide the empirical ground. In ***Philosophy of Logic***[387] (1970), he tells us that observation sentences are "conditioned" to observation. They are "learned **ostensively**; they are learned in the situation that they describe, or in the presence of the things that they describe."[388] Observation sentences must be conditioned to publicly shared observations "since both teacher and learner have to see the appropriateness of the

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387 W.V.O. Quine, ***Philosophy of Logic***, (Englewood Cliffs, N.J., 1970).

388 ***Ibid***., p. 6.

occasion."[389] Everyone in a particular community will learn these expressions in similar ways and thus they will tend to apply these expressions uniformly in the presence of the same stimulations. He says that "it is because of this uniformity ... that scientists who are checking one another's evidence gravitate to observation sentences as a point where concurrence is assured."[390] In the face of this, we can conclude that Frankfurt cannot call Quine on his lack of empiricism.

We can, however, along with Frankfurt, call Quine on his failure to develop the relationships between his basic sentences and the other sentences of the language. In his later book, ***Philosophy of Logic***, he does not fill out this relationship either. All that he tells us is that expressions other than the observation sentences are learned "contextually in ways that generate a fabric of sentences, complexly interconnected."[391] Further, he writes:

> The connections are such as to incline us to affirm or deny some of these sentences when inclined to affirm or deny others. These are the connections through which a theory of nature imbibes its empirical substance from the observation sentences. They are also the connections whereby, in an extremity, our theory of nature may tempt us to ignore or disavow an observation, though it would be regrettable to yield often to this temptation.[392]

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[389] ***Ibid***.

[390] ***Ibid***.

[391] ***Ibid***.

[392] ***Ibid***.

Again, he reiterates the point of the D-thesis, for he tells us that "our theory of nature is under-determined by all 'possible' observations."[393] The point being", as we have seen before, that there is no one true theory, but rather there is a surfeit of theories which explain the observations. This is the indeterminacy that Quine expresses in ***Word and Object***. (There he calls it the indeterminacy of translation). In ***Philosophy of Logic***, he puts it as follows:

> This means that there can be a set H of hypotheses, and an alternative set H' incompatible with H, and it can happen that when our total theory T is changed to the extent of putting H' for H in it, the resulting theory T' still fits all possible observations just as well as T did. Evidently then H and H' convey the same empirical information, as far as empirical information can be apportioned to H and H' at all: but still they are incompatible.[394]

Quine provides a clue which may help us to determine a way of ferreting out the fabric of science. He tells us that scientists employ a strategy which he calls "a maxim of minimum mutilation."[395] On this strategy one tends to alter the more empirical sentences first in the face of a disconfirmation. He gives us the following example:

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393 ***Ibid***.

394 ***Ibid***., pp. 6-7.

> In ***Word and Object*** the indeterminacy thesis is presented as follows:
>
> The thesis is then this: manuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another. In countless places they will diverge in giving, as their respective translations of a sentence of the one language sentences of the other language which stand to each other in no plausible sort of equivalence however loose. The firmer the direct links of a sentence with non-verbal stimulation, of course, the less drastically its translations can diverge from one another from manual to manual. (***Word and Object***, p. 27.)

395 ***Phil. of Logic***, p. 7.

> Thus suppose from a combined dozen of our theoretical beliefs a scientist derives a prediction in molecular biology, and the prediction fails. He is apt to scrutinize for possible revision only the half dozen beliefs that belonged to molecular biology rather than tamper with the more general half dozen having to do with logic and arithmetic and the gross behaviour of bodies.[396]

The point is similar to the one raised in "Two Dogmas" about analytic sentences. The reason why there seem to be sentences that carry their entire empirical significance is not because there are such sentences, but only because we behave as if there were such sentences in order to keep as much of our theory intact as possible. Of course, if we are going to exercise a maxim to keep our theory as intact as possible, we need to know the weave structure of our theory or language. To know this would be to know the answers to some of the questions that have perplexed us. How do we know which sentences to preserve and which sentences to attack? How do we determine what alterations cause a minimum of mutilation? How does a scientist know which of his hypotheses to single out for change?

Quine tells us that the molecular biologist "will not even confront the six beliefs from molecular biology impartially with the failure of prediction; he will concentrate on one of the six, which was more suspect than the rest."[397] This is quite an usual procedure in normal science. As Quine puts it:

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[396] ***Ibid***.

[397] ***Ibid***.

> Scientists are indeed forever devising experiments for the express purpose of testing single hypotheses; and this is reasonable, insofar as one hypothesis has been fixed upon as more tentative and suspect than other parts of the theory.[398]

But the question remains: How does the scientist know which hypothesis to fix "upon as more tentative and suspect than other parts of the theory.?" What guides him in his direction of attack? Why do we consider beliefs "having to do with logic and arithmetic and the gross behaviour of bodies" as the last to be given up in the face of recalcitrant experience?

Since there are complex interconnections within the fabric of science, the matter is more complicated than just selecting out one hypothesis for change. Other sentences go along with the rejected hypothesis. If one rejects an hypothesis, then one must reject any which imply it.[399] Even granting all this, that we are not just rejecting single hypotheses but rather whole sets of interconnected hypotheses, how do we determine which set to dispense with in the face of recalcitrant experience?

The clue that Quine has provided is that we look to logic to provide the connections between various sentences of the system. While it is not the business of logic to determine which simple sentences are true, it is the business of logic to determine given the truth or falsity of the simple sentences which compound sentences will be true or false, or given the truth or falsity of compound

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398 ***Ibid***.

399 ***Ibid***.

Quine adds that he does not wish to "lean on a notion of implication" (***Ibid***., p. 7.) for he is challenging the notion of implication and the notion of equivalence or mutual implication.

sentences to determine what alternatives remain open for the simple sentences. We shall return to this later on when we will elaborate on a suggestion for getting at the internal fabric of the system of science. For the present, let us consider Frankfurt's other criticisms, and see how they may help us in teasing out the implications of the D-thesis.

Frankfurt's Other Criticisms

In his paper, Frankfurt claims that there is "an apparent contradiction"[400] between Quine's theory about truth and Quine's theory about meaning. On examination of Frankfurt's complaint, we can see that it bears some resemblance to Grünbaum's point about the semantic stability of the theoretical language. According to Frankfurt:

> Quine's doctrine appears to involve (a) the theory that changes in the system to which a statement belongs alter the statement's meaning - that is, turn it into a different statement; and also (b) the theory that the same statement that was true (or false) before such changes may be false (or true) after they have been effected. But these theories are incompatible because (a) asserts that a change in the context of a statement makes it a different statement, while (b) requires that a statement remain the same despite changes in its context. In order for the truth value ascribed to a statement to change, the statement whose truth-value is at issue must be the same statement before and after the change. Otherwise the "change in truth-value" is simply the substitution of a statement with one truth-value for a different statement with a different truth-value.[401]

In other words, Quine's theory of meaning sets the unit of empirical significance as the whole of science rather than as the statement. Thus the meaning of a statement (here we follow Frankfurt in

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[400] Frankfurt, ***op. cit***., p. 174.

[401] ***Ibid***., p. 175.

his use of 'statement'] is determined by its relationship to the whole system of science. As Frankfurt puts it: "the meaning of any statement depends upon the context provided by other statements of the system to which it belongs."[402] Thus when one makes the necessary changes elsewhere in the system in order to save a particular statement, one is changing the context of the statement and thus the meaning of the statement in question. For example, in the case of the statement "Ordinary buttermilk is toxic," it is no longer the same statement if we change the intension of 'buttermilk' to that of 'arsenic' in order to preserve it. The meaning has been changed to the one ordinarily given by the statement "Ordinary arsenic is toxic." Frankfurt's claim is that this conflicts with Quine's theory of truth which apparently claims that it is the same statement that is true (or false) after the change that was false (or true) before the change. Quine cannot have it both ways.

Certainly if one construes Quine's theory in the way that Frankfurt does, one comes up with a contradiction. Naturally, Quine would not want to construe matters in that manner. He would object to Frankfurt's interpretation. As Quine points out in his letter to Frankfurt:

> Another difficulty that you raised was that since I referred all meaning to the whole system, I retained no way of making sense of sameness of statements. In this connection I would clarify first a minor point of terminology: in my own writings, early and late, 'statement' has always referred to linguistic forms, and not to their meanings. This may have thrown you off in specific passages, but your general point remains important, and it is one that I recognize and discuss in ***Word and Object***, top. p. 24 and elsewhere.[403]

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[402] ***Ibid***., p. 174.

[403] see letter in footnote 13.

From the D-theoretic point of view, what one may be doing (and one may be doing other things as well) when one changes the system in order to save a particular sentence[404] is altering the system so that the meaning of the particular sentence changes to fit the experience. If we revert to the formulation of our third chapter, what we are doing is changing the system from (H & A) to (H & A′), where the system (H & A′) provides the proper prediction or explanation. In making this change the meaning of H does not remain stable but changes. It is certainly the case that in making the appropriate alterations the D-theorist may be substituting a statement with one truth-value for a different statement[405] with a different truth-value. According to the D-thesis, truth does not apply to sentences individually, but rather to the whole system in which the sentence is contained. By making the appropriate alterations in the system one not only changes the system, but also one changes the meaning of sentences within the system.

[404] I use the word 'sentence' here to avoid the ambiguity found in the use of 'statement' in this context. 'Sentence' here refers to a well-formed string of words within a particular language without regard to meaning. "Mary had a little lamb" is a sentence, as well as "The absolute is each appearance, and is all, but it is not any one as such" (F.H. Bradley, ***Appearance and Reality*** (Oxford, 1897) p. 131 - example from Achinstein, ***Concepts of Science*** op. cit. p. 75.) "Grass the is" is not a sentence.

[405] Here, of course, I am using 'statement' to refer to the meaning of a sentence. Just as "It is raining" and "Il pleut" have the same meaning and hence make the same statement even though they are sentences of different language, one can also use the same sentence to make two different statements; for example "John has red hair" where, in fact, one is referring to a John Smith who has red hair - in which case the statement is true, or "John has red hair" where one is referring to a John Henry who has black hair - in which case the statement is false. Some philosophers would refer to these by the term 'proposition' rather than 'statement', but I will ignore that dispute here.

Frankfurt's Suggestions:

Frankfurt has made a few suggestions to get Quine out of his 'inconsistency'.[406] However, besides arising out of a misconception of the D-thesis, these suggestions are rather weak and leave Frankfurt's interpretation of the D-thesis still subject to a criticism of inconsistency. First, he suggests to Quine: "... that he could avoid the contradiction in his views... by allowing that **some** changes in the context would be so insignificant as to leave the statement's original meaning intact."[407] If Frankfurt's construal of Quine's thesis were correct (and it is not), this suggestion, besides removing any force or usefulness remaining in the doctrine, would leave the matter open to a dispute about whether the changes were significant or insignificant.[408] There would be enough trouble caused by such a change without having to define something as vague as "significant enough". As Frankfurt, rightly, points out, in the "Two Dogmas" paper Quine did permit the changes to include "revisions of the laws of logic and mathematics."[409] Certainly, these changes are so drastic as to significantly affect a great number of ordinary statements. As Frankfurt points out in a footnote:

> I suppose it is at least a necessary condition of two statements having the same meaning that the logical consequences of the two statements should coincide. But if the principles of inference are altered, then the set of logical consequences of any given statement is also

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406 I have put 'inconsistency' in quotes since I have already shown that the inconsistency only arises if you misinterpret the D-thesis in the way that Frankfurt has.

407 Frankfurt, ***op. cit***., p. 175.

408 A similar dispute was also found to be possible in connection with Grünbaum's condition of semantic stability. The question arises, "How does one decide whether a change is semantically relevant or non-semantically relevant? According to Achinstein, one could do this by asking the scientists (in the case of scientific changes). **But** the problem is where do you stand when the scientists disagree for theoretical reasons?

409 Frankfurt, ***op. cit***., p. 175.

altered. Thus the laws of logic have a peculiarly intimate connection with the meanings of statements.[410]

It is very, clear, then, that Quine wishes to permit changes that are 'significant enough'. He would certainly not go along with limiting the scope of his theory in the sense suggested by Frankfurt. Particularly, Quine would reject Frankfurt's suggestion that he "concede that there are some statements whose meanings are not dependent on their contexts."[411] To concede that would be to go back on his pronouncement on the analytic- synthetic distinction. Similarly, Quine would reject Frankfurt's alternative suggestion that: "If he wishes to go on maintaining the meaning of every statement is contextually determined, he will have to concede that there are some statements whose truth-values are irrevocably fixed."[412] The same vagueness as we found in Frankfurt's previous suggestion arises in this case, for as Frankfurt points out: Quine "will also have to admit that compensatory changes required in order to alter a statement's truth value are sometimes so insignificant as to leave the meaning of the statement unaffected."[413]

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410 ***Ibid***., p. 176.

411 ***Ibid***., p. 176, and Quine "Two Dogmas..." p. 43.

412 ***Ibid***., p. 176.

413 ***Ibid***.

It is quite clear that Quine would reject the suggestion of having sentences[414] with irrevocably fixed truth-values. Although he would allow that in certain systems certain sentences are fixed or analytic in the system - that is, they are sentences that are given up last, he certainly would not hold that they are irrevocably fixed. He wrote in "Two Dogmas" that "... no statement is immune from revision."[415] Following his explanation in his letter to Frankfurt, this means that no linguistic form is immune from revision.

Frankfurt's suggestions come about as a result of an attempt to get out of the 'supposed' contradiction between Quine's theory of meaning and Quine's theory of truth. Clearly, Frankfurt's suggestions go counter to a great deal of what Quine wishes to hold. It is also clear that Quine's position need not result in the contradiction that Frankfurt attributes to it. The obvious conclusion is that Frankfurt's construal of Quine's position is a misinterpretation.

Meaning Change:

Following Quine's correction of Frankfurt a restatement of the D-thesis can be made which makes it quite clear that a meaning change is of the essence of the D-thesis. Under Quine's use of

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414 Again I use the word 'sentences' here to try to avoid the ambiguity caused by Frankfurt's and Quine's use of the word 'statement'. A statement is a particular kind of sentence with a meaning. (I remind the reader that not all sentences are statements. There are commands, questions, explanations, etc.) Thus it is sentences that are preserved by the D-thesis. The linguistic form of a particular H remains the same, although, as I have pointed out, the meaning of the H may change when placed in the context of (H & A'). Thus the H after the change may not make the same statement as before the change. Together the whole system, (H & A) will be true or false. Given the new context, we will be able to determine whether sentence H makes a true statement or a false statement within that context. This will be discussed later in the thesis.

415 Quine, "Two Dogmas..." ***op. cit***., p. 43.

statement, the D-thesis reads: "Any linguistic form can be made to represent a true sentence come what may, if we make drastic enough adjustments elsewhere in the system." The implication is that the linguistic form can have many different meanings depending on the system in which it is embedded. Thus a given sentence (linguistic form) can have a true interpretation in one system and a false interpretation in another system. Thus that sentence can be retained as a true sentence in the face of evidence to the contrary by appropriately altering the system in which the sentence is found. Not only does the meaning of the sentence change, but also we have a change in the system in which the sentence is found.

Under Quine's D-thesis, meaning change in science is permissible. Quine is fully aware of the implications of this claim. In particular, it lies at the basis of his rejection of the analytic-synthetic distinction as a distinction in kind. His argument in "Two Dogmas..." was that modern empiricism was grounded on two ill-founded dogmas, the analytic-synthetic cleavage, and the dogma of reduction. The D-thesis effectively undermines these two. Quine indicated at the outset of his paper that he was aware that acceptance of the D-thesis would blur "the supposed boundary between speculative metaphysics and natural science"[416], and also lead to a "shift toward pragmatism."[417] Certainly the permission of meaning change results not only in blurring the supposed boundary between speculative metaphysics and science but demolishes it. Without the appropriate restraints imagination would be the limit. It will be clear from what follows that the constraints which must be placed on scientific theorizing in the wake of permitting meaning change are pragmatic constraints.

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[416] ***Ibid***., p. 20.

[417] ***Ibid***.

The development of these constraints will be a step towards what Quine calls "Empiricism without the dogmas."[418]

Quine tempers the D-thesis with pragmatism. What the D-thesis lets loose, pragmatism reins in. The D-thesis opens the door to speculation and Quine's pragmatic constraints hold it in line.

Frankfurt correctly recognized that in Quine's theory, "Experience can be accommodated by any one of a number of different systems of beliefs, and any given statement will belong to some of these alternatives systems and will be excluded from others."[419] This fact does not take away from the empirical nature of Quine's theory. The empirical element is imported insofar as a given system has its chosen set of observation sentences which are 'conditioned' to observations (experience). Given any set of data, there can be many theories that entail the sentences that describe the data. The data do not distinguish between the theories. People who believe in witches and other such occult phenomena (and even gods or God) are very adept at showing how their particular theories are borne out by experience. Anyone who has ever tried to argue with these particular individuals soon realizes that among these who sincerely believe there is an elaborate belief system by means of which they can show how certain empirical phenomena can be explained by their particular set of beliefs. No amount of arguing can convince them otherwise since all that one can do is suggest another belief system which also entails the data. Both systems of belief can be said to be borne out by experience,

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[418] ***Ibid***., p. 42.

[419] Frankfurt, ***op. cit***., p. 174. Again for 'statement', we should read 'sentence' or 'linguistic form' or 'typographical form'.

so on the basis of experience alone one cannot decide between the belief systems. One has to resort to other features of the belief sets. It is not the empirical nature of a belief system that is the crucial issue, and this is the point of the D-thesis.

Given the acceptance of the D-thesis, the crucial question for science becomes: "How can one decide between equally empirical alternative belief systems?" The D-thesis tells us the we can hold any hypothesis to be true by erecting an elaborate enough belief system such that the hypothesis and the appropriate background theory will entail true observation sentences of the theory or belief system. How do we decide between competing theories? Presumably, some theories in science are better than others; How do we decide which is better? On what basis do we decide? On what basis ought we to decide? Is there a set of reasons why we prefer to talk in terms of physical objects, electrons, molecules, etc., and disparage talk about witches and demons?

To import Chomsky-like language, the surface structure of the data is indifferent to the possible deep structures (theories) that explain the data. How do we decide between the deep structures?

Frankfurt has claimed that "it is at least a necessary condition of two statements having the same meaning that the logical consequences of the two statements coincide."[420] Under such an assumption, the meaning of an expression is a function of its logical consequences. Quine's D-thesis

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[420] Frankfurt, ***op. cit***., p. 176.

sets the meaning of a sentence to be dependent on the system of sentences of which it is a member. Its meaning is determined by its complex interconnections with other sentences of the system.

Certain problems of comparability arise when one takes the matter of meaning too far. An awareness of these problems, I think, lies behind Grünbaum's demand for semantic stability[421] and some of Frankfurt's criticisms. The problem of meaning variance has prompted numerous papers recently.[422] Most of this literature has been in response to certain views held by T.S. Kuhn[423] and

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421 Achinstein's concepts of relevance derive from an attempt to cope with meaning variance. Achinstein criticized Feyerabend's thesis in his paper, "On the Meaning of Scientific Terms", ***Journal of Philosophy*** vol. 61. (1964) pp. 497-509. His book, ***Concepts of Science*** is certainly an attempt to fill out the points made in that paper.

422 **A Brief (and incomplete) Bibliography on Meaning Change:**

P. Achinstein, "On the Meaning of Scientific Terms", ***Journal of Philosophy*** vol. 61 (1964) pp. 497-509.

P.K. Feyerabend, "On the "Meaning" of Scientific Terms", ***Journal of Philosophy*** vol. 62 (1965) pp. 266-271.

D. Shapere, "Meaning and Scientific Change", ***Mind and Cosmos***, ed. R.G. Colodny, (Pittsburgh, 1966) pp. 41-85.

A.I. Fine, "Consistency, Derivability, and Scientific Change", ***Journal of Philosophy*** vol. 64 (1967) pp. 231-240.

M.E. Levin, "Fine's Criteria of Meaning Change", ***Journal of Philosophy*** vol. 65 (1968) pp. 46-56.

M.B. Hesse, "A Self-Correcting Observation Language", ***Logic, Methodology, and Philosophy of Science*** III. ed. B. Van Rootselaar and J.F. Staal (Amsterdam, 1968) pp. 297-309.

J. Leplin, "Meaning Variance and the Comparability of Theories", ***British Journal for the Philosophy of Science***, vol. 20.(1969) pp. 69-80.

J. Giedymin, "The Paradox of Meaning Variance", ***British Journal for the Philosophy of Science***, vol. 21 (1970) pp. 257-268.

A.K. Feyerabend.[424] It would be an unnecessary diversion to discuss the particular views of these

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K.P. Parsons, "On Criteria of Meaning Change", ***British Journal for the Philosophy of Science*** vol. 22 (1971) pp. 131-144.

L.R. MacCormac, "Meaning Variance and Metaphor" ***British Journal for the Philosophy of Science*** vol. 22 (1971) pp. 145-159.

[423] In the main, these are views that he expresses in his book, ***The Structure of Scientific Revolutions***, 2nd ed. enlarged, (Chicago, 1970).

[424] Bibliography of some of Feyerabend's papers:

P.K. Feyerabend, "An Attempt at a Realistic Interpretation of experience", ***Proceedings of Aristotelian Society***, New Series, 58 (1958) pp. 143-170.

Knowledge without Foundations (Oberlin College, 1961).

"Explanation, Reduction, and Empiricism", ***Minnesota Studies in the Philosophy of Science***, III, ed. H. Feigl and G. Maxwell (Minneapolis, 1962) pp. 28-97.

"Problems and Microphysics" ***Frontiers of Science and Philosophy*** ed. R.G. Colodny (Pittsburgh, 1962) pp. 189-283.

"Problems of Empiricism", ***Beyond the Edge of Certainty***, ed. R.G. Colodny (Englewood Cliffs, 1965) pp. 145-260.

"Reply to Criticism", ***Boston Studies in the Philosophy of Science***. II ed. R.S. Cohen and M.W. Wartofsky (New York, 1965) pp. 223-261.

"On the "Meaning" of Scientific Terms" ***Journal of Philosophy*** vol. 62 (1965) pp. 266-274.

"Classical Empiricism" The Methodological Heritage of Newton ed. R.B. Butts (Toronto, 1970) pp. 150-170.

"Against Method" ***Minnesota Studies in the Philosophy of Science*** IV ed. M. Radner & S. Winokur (Minneapolis, 1970) pp. 17-130.

"Consolations for the Specialist", ***Criticism and the Growth of Knowledge*** ed. I Lakatos & A. Musgrave (Cambridge, 1970) pp. 197-230.

philosophers. However, we will consider some of the paradoxes that derive from their particular views especially with respect to their applicability to the D-theoretic point of view.

The Paradoxes of Meaning Variance

If one adopts a context theory of meaning (whereby the meaning of a sentence is dependent on the context provided by the theory of which it is a part) as a sufficient account of the meaning of a sentence, then certain paradoxes arise when one attempts to talk about change of meaning in sentences and terms. Earl MacCormac presents one paradox as follows: "it would be impossible to understand a new theory since the new terms in it would be unrelated to the meanings they had in previous theories."[425] He explains that this problem arises because one can never break into the circle of meaning. Since the meaning of the new terms rest upon the context of the theory one can never know the terms without understanding the theory, but, on the other side, one can never understand the theory without understanding the meaning of the constituent terms. MacCormac presents a second paradox as follows: "if all the terms of a new theory had new meanings, then it would be impossible to show that two theories were logically incompatible since the same term appearing in both theories would have a different meaning in each."[426] Difficulties also arise when we try to compare the meanings of terms by means of some third language, say a metalanguage. Feyerabend thought that one way around the paradoxes might be to invent a theory more general than each of the theories being compared. This more general theory would describe "a common background that

425 Earl R. MacCormac, "Meaning Variance and Metaphor" ***British Journal for the Philosophy of Science*** vol. 22 (1971) p. 145.

426 ***Ibid***.

defines test statements acceptable to **both** theories."[427] Dudley Shapere has shown that this attempt fails because the meanings of the statements in the metatheory will be different again from the meaning of the corresponding sentences in the original theories even though they appear to be exactly alike:- the contexts are different in each case.[428]

Another procedure that Feyerabend suggests for moving around the paradoxes is based on what he calls "an internal examination."[429] This examination presumably enables one to determine which theory has the more direct connection to observation. Shapere confesses that he does not understand what Feyerabend intends because "...each theory defines its own facts or experience"[430] and nothing could be more direct than that. Such a procedure as Feyerabend recommends would be of no real help to the D-theorist who, at times, will be concerned with a means of deciding between equally empirical theories.

Feyerabend's third suggestion uses a pragmatic theory of observation as a background for choosing between theories. Feyerabend's point is that human experience is available for comparison and testing of theories. He says:

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427 D. Shapere, "Meaning and Scientific Change" **Mind andCosmos** ed. R.G. Colodny (Pittsburgh, 1966) p. 58. (See Feyerabend, "Problems of Empiricism" (P of E) pp. 216-217.)

428 ***Ibid***.

429 ***Ibid***., p. 59. (See Feyerabend "P of E" p. 217.)

430 ***Ibid***.

> It is bound to happen, then at some stage, that the alternatives do not share a single statement with the theory they criticize. The idea of observation that we are defending here implies that they will not share a single observation statement either. To express it more radically, each theory will possess its own experience, and there will be no overlap between these experiences. Clearly, a crucial experiment is not impossible. It is impossible not because the **experimental device** would be too complex or expensive, but because there is no universally accepted **statement** capable of expressing whatever emerges from observation. **But there is still human experience as an actually existing process**, and it still causes the observer to carry out certain actions for example, to utter sentences of a certain kind.[431]

Problems arise with this suggestion because human experience comes categorized. If we could be assured that everyone categorized in the same way, then, perhaps, there might be a common experience that could be appealed to. However, the issue is not all that clear. Can we be assured that human beings are equipped with a similar set of categories? Is our basic set of categories a function of our particular language or is there some set which is common to every man irrespective of his socio-cultural origins? One way that philosophers have attempted to get around the problems that arise when talking of a common experience is to talk about the 'given' or the uncategorized aspect of our experience that is independent of our thought. Essentially, though, that is to trade one set of difficulties for another. The problem with talk about a 'given' is that one can never escape one's categories of experience and know some category-free 'given'. This is what Scheffler calls the **paradox of categorization**.

> If my categories of thought determine what I observe, then what I observe provides no independent control over my thought. On the other hand, if my categories of thought do not determine what I observe, then what I observe must be uncategorized, that is to say, formless and nondescript - hence again incapable of providing any test of my thought. So in neither

[431] ***Ibid***. (See Feyerabend, "P of E", pp. 214-215)

> case is it possible for observation, be it what it may, to provide any independent control over thought.[432]

To put it in Kantian language, we are caught in a phenomenal world and we can never get behind the phenomena to the noumenal world, that is, to the world uncategorized. The categories are necessary to having any experience at all. The point is that there is nothing that is theory independent on the basis of which to use Feyerabend's third suggestion. One way out would be to admit that men do categorize in the same way (creating a common experience) and showing that this categorization can be indifferent to alternative theories.

In addition to the paradox of categorization, Scheffler lists two other paradoxes which are similar: the paradox of common observation and the paradox of common language. The former states that if observation is theory-laden, there can be no neutral observations capable of deciding between two different theories.[433] Similarly, the latter states that there cannot be a neutral account on the basis of which one can compare alternative theories; basically, because the observational derivations of

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[432] I. Scheffler, ***Science and Subjectivity*** (New York, 1967) p. 13.

[433] ***Ibid***., p. 15.
The paradox of common observation goes as follows:

> "For if seeing is indeed theory-laden in the sense described, then proponents of two different theories cannot observe the same things in an effort to resolve their differences; they share no neutral observations capable of deciding between them. To judge one theory as superior to the other by appeal to observation is always doomed, therefore, to beg the very question at issue." (***Ibid***.)

each theory will have a different meaning regardless of any resemblance they may have linguistically.[434]

Any position that leads to such paradoxes is basically wrong. Such a position denies that there can be a community of science - a claim that is obviously mistaken. Such a position denies that we can compare theories according to their handling of empirical facts. It denies that one can compare alternative theories within the same domain because such theories cannot share a domain. It denies cumulative growth to science and leaves each scientist isolated within his own realm of meanings. All of which runs counter to obvious facts. Scheffler claims that if such a position is seriously put forward for consideration, it is basically self-contradictory. He writes:

> But to put forth **any** claim with seriousness is to presuppose commitment to the view that evaluation is possible, and that its favours acceptance; it is to indicate one's readiness to support the claim in fair argument, as being correct or true or proper. For this reason, the particular claim that evaluation is myth and fair argument a delusion is obviously

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[434] ***Ibid***., p. 17.

The paradox of common language is:

"It seems to follow, further, that we cannot literally speak of alternative theories **of the same domain**, nor of comparing these theories to see which gives a better account of the empirical facts within this domain. For there is not, and there cannot be, a neutral account of the domain in question, since the observational derivations of each theory differ in meaning from those of the other, no matter how similar they are as sound patterns, Nor can one law really be absorbed into another through a process of reduction, nor observational content passed on from one theory to its successor, for crucial meaning changes have occurred in the process of transfer. We have here another paradox, the **paradox of common language** and its upshot is that there can be no real community of science in any sense approximating that of the standard view, no comparison of theories with respect to their observational content, no reduction of one theory to another, and no cumulative growth of knowledge, at least in the standard sense. The scientist is now effectively isolated within his own system of meanings as well as within his own universe of observed things." (***Ibid***.)

> self-destructive. If it is true, there can be no reason to accept it; in fact, if it is true, its own truth is unintelligible: what can truth mean when no evaluative standard is allowed to separate it from falsehood? And indeed there is a striking self-contradictoriness in the effort to persuade others by argument that communication, and hence argument, is impossible; in appeal to the facts about observation in order to deny that commonly observable facts exist; in arguing from the hard realities of the history of science to the conclusion that reality is not discovered but made by the scientist. To accept these claims is to deny all force to the arguments brought forward for them. Acceptance implies that we are free to assert what we will and hence, in particular, to reject them.[435]

Clearly, any view that leads to the paradoxes of categorization, common observation, and common language flies in the face of matters that are part of common everyday experience. We can and do communicate; we can and do put forward arguments in a serious effort to convince; we can and do compare theories; and we can and do determine meaning changes. Since such views fail to explain these common everyday experiences, they must be condemned. Scheffler's main point is that a contextual theory of meaning by itself fails to be a sufficient account of meaning.

An unbridled form of the D-thesis is subject to all the complaints against a contextual theory of meaning. A D-theorist, however, is concerned to have a science where theories can be evaluated and compared. He realizes that in order to avoid the undesirable consequences of a purely contextual theory of meaning, the D-thesis must be reined in or limited in some way. If we follow the metaphor, we can say that it is pragmatism that puts the reins on the D-thesis. That is, the D-thesis opens the way for speculation, but it must be held in line by thorough-going pragmatism. There is good traditional support for this move since C.S. Peirce considered pragmatism to be the logic of

[435] ***Ibid***., pp. 21-22.

abduction (hypothesis)[436] and since the D-thesis applies directly to the question of the control of hypothetical reasoning.

Before we consider the pragmatic constraints on the D-thesis, it is important that we examine Scheffler's solution for finding an objective basis for communication, argument, theory comparison, evaluation, and the like. Scheffler's attempt to avoid the paradoxes succeeds in a fashion that is useful for the development of the D-theoretic point of view. He does not develop a theory-neutral observation language (such as the positivists advocated), but rather his approach might easily be summed up as follows: "Men agree in order that they may disagree."

The question is where men must agree in order to establish a basis for communication, etc. Grünbaum insisted on semantic stability. In the preservation of a given hypothesis, one was not permitted to change the meaning of the hypothesis. Meaning change was **forbidden**. The basis of comparison and communication lay in the stability of the meaning relationships within the theory. This, we believe, was an unnecessary restriction on the D-thesis. Historical examples cited by Hesse, Kuhn, Feyerabend and even Grünbaum himself indicate that very important advances in scientific endeavour involved semantic shifts. Theories do differ, meanings of terms and sentences in theories

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[436] C.S. Peirce, ***Collected Papers of Charles Saunders Peirce*** vol. V (Boston, 1965) 5.196 (The 196 refers to the paragraph number).

Peirce wrote: "If you certainly consider the question of pragmatism you will see that it is nothing else than the question of the logic of abduction. That is, pragmatism proposes a certain maxim which, if sound, must render needless any further rule as to the admissibility of hypotheses to rank as hypotheses.... For the maxim of pragmatism is that a conception can have no logical effect or import differing from that of a second conception except so far as, taken in connection with other conceptions and intentions, it might conceivably modify our practical conduct differently from that second conception.

do differ. Progress in science depends on the possibility of differing theories. The answer does not seem to lie in firming up the deep structures. That can only lead to stagnation in science.[437] A better choice for agreement is at the level of observation, the surface structure. We do share natural languages and are conditioned in their use very early in life. While all sentences are theory-laden to some extent (including observation sentences) there are some sentences that are less so - the observation sentences. These are conditioned to experience, and hence, a behaviourist aspect of meaning is added.

There are two aspects to the meaning of sentences in a particular theory: the contextual aspect or the role of the sentence within the theory, and the stimulus aspect, the stimulus meaning. Just as a reference theory of meaning cannot provide a sufficient account of meaning, so also a context theory of meaning fails to be sufficient. Now an account which includes both aspects is much more fruitful. The contextual part provides the depth and since stimulus meaning can be shared meaning, one looks there for the base of communication, theory comparison and the rest.

Scheffler's Solution:

Scheffler's first paradox, the paradox of categorization, brings in an extremely tangled web of problems. We do not wish to go into detail here on the question of categories. It will have to

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[437] This statement is, perhaps, a little extreme, for certainly there is room for semantic stability in normal science (to use Kuhn's language). In fact, semantic stability is necessary in a situation where one is filling out the implications of a particular paradigm. Normal science, however, is only part of science and if semantic stability were insisted upon at all levels of science then it would tend to stagnate in one paradigm. Progress in the sense of paradigm shift cannot be made if one insists upon semantic stability.

suffice for the present to make a number of assumptions. We will assume that men can and do categorize in similar ways. Why this is so will remain to be explained.[438] The basic reason for making this assumption is the recognition that we do communicate with our cultural peers, and at least at the level of ordinary language we do seem to talk about the same kind of objects, etc. If we did not categorize in the same way or, in other words, have common experiences, we could not communicate. Scheffler points out in his discussion of the paradox of categorization that alternative

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[438] There are a number of answers to this question. Two such answers are: (1) It may be that the structure of the human mind is such that we categorize in the way that we do, that is, they are in a sense innate - we are born with certain faculties; (2) it may be that we are conditioned by our culture to categorize in a certain way, that is, the languages we learn (verbal or otherwise) determine a basic categorization which native users of the language internalize. These need not be exclusive alternatives for one can imagine a situation where our peculiar categorization is a function of both the structure of our mind and a function of our language.

> Sometimes it is argued that different languages in the world have different conceptual bases and this seems to be a product of different ways of categorizing - (the Hopi Indian language is often brought up in this connection). This is brought forward in support of the second answer. However, this is not decisive; in response it is pointed out that we can translate from one language to another. This fact shows that there can be some sort of correlation between the two languages, and this provides evidence that there must be some categorization basic to the two which permits the correlation to be made. Also it is pointed out that children from different cultures can be brought up as native speakers of any other human language. This latter fact provides support to the view that there is a human capacity which equips us for an understanding of any human language, - hence, a shared faculty. Such a faculty could be part of our evolutionary heritage. (This raises the question of whether or not we might be able to communicate with intelligent non-human life.]
>
> Quine has suggested a kind of evolutionary origin for certain of our categories. He writes:
>
> ...why does our subjective spacing of qualities accord so well with the functionally relevant groupings in nature as to make out inductions tend to come out right? Why should our subjective spacing of qualities have a special purchase on nature and a lien on the future?
>
> There is some encouragement in Darwin. If people's innate spacing of qualities as a gene-linked trait, then the spacing that has made for the most successful inductions will have tended to predominate through natural selection. Creatures inveterately wrong in their inductions have a pathetic but praiseworthy tendency to die before reproducing their kind. (Quine, "Natural Kinds", ***Ontological Relativity and Other Essays***, (New York, 1969) p. 126.)

theories can be presented within the same categorical structure. He compares a category system with a classification system; his example is an alphabetical filing system for correspondence. He writes; "Categorization provides the pigeonholes; hypothesis makes assignment to them."[439] Thus a given categorical system does make a determination as to how experience is to be organized, but it does nor prejudge "our hypothesis as to the actual distribution of items within the several categories."[440] Scheffler believes that a category system is akin to vocabulary and grammar in a language: "Without a vocabulary and grammar, we can describe nothing; having a vocabulary and grammar, our descriptions are not thereby determined."[441]

Certainly, a particular categorization, though, will exclude hypotheses that conflict with its particular 'classification'. The only alternative hypotheses that will be allowed are those that are expressible within that particular categorization. This need not be a problem for us, however, for if it is assumed that we all categorize in the same way, then these other hypotheses will not be relevant to other types

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[439] Scheffler, ***op. cit***., p. 38.

[440] ***Ibid***.

[441] ***Ibid***.

"A category system, within a limited context, may be described as imposing order in general and in advance on whatever experience in that context may bring. It commits us to ways of delimiting items to be recognized, as well as to modes of classifying them. Lacking such order altogether, we may, indeed, aptly be described as facing undifferentiated chaos, since we lack the very recognition of things -that is to say, we do not individuate and separate items as objects of reference. Yet having a category system, we do not thereby prejudge the manner in which we shall need to apply it in the future. Without a vocabulary and grammar, we can describe nothing; having a vocabulary and grammar, our descriptions are not thereby determined." (***Ibid***.)

of experience and further they will not even be expressible. We are concerned here only with science as conducted by humans.[442]

Let us return to the matter of the context theory of meaning. Further evidence of its inadequacy as a theory of meaning comes when we consider the question of how a child learns a language. If things were as described in the context theory of meaning, then a child could never get started on a language. For that matter, no one could start to learn a language. If the meaning of terms and sentences in a language were only determined by the context of the whole language and the whole complex of relationships between the terms and sentences of the language, then there would be no way to start learning a language except by somehow grasping the whole language in one fell-swoop. This shows that the context theory of meaning is not the whole story; it is insufficient as an account of meaning.

As Scheffler points out, the context theory of meaning ignores the referential aspect of meaning. It concentrates on just the senses of terms and sentences. It ignores the denotative aspect of language and confines its view of meaning to the connotative aspect. A purely connotative theory

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[442] I grant that this is a sticky question and that the assumption, at least from a philosophical point of view, is a rather large one. It is a questionable assumption. A detailed examination of the question of categories could very well lead to an answer that would undermine my position. Be that as it may, I will carry on my project to determine the philosophical significance of the D-thesis. Perhaps, it might be wise to retitle the dissertation, "The Philosophical Significance of the D-thesis on the Assumption that Humans Categorize in the Same Way."

of meaning is not sufficient to account for meaning; nor, I might add, is a purely denotative account.[443] Meaning, as has been traditionally recognized, has this dual aspect: sense and reference.

It is precisely that we can and do learn languages that shows a purely connotative theory of meaning to be insufficient. Scheffler writes:

> Indeed, were it not possible to gain an understanding of words in application independently of a prior elaboration of their meanings in understandable terms we should be at a loss to explain how such understanding is acquired in the first instance. Were the child never to understand any word before grasping some synonymous defining expression he could never begin to understand any word at all. For each such defining expression would itself require a further synonymous expression in order to be understood, and, barring unilluminating circularity and infinite regress in the chain of synonymies, the initial expression would, in every such chain, remain beyond his reach: the process could, in short, never get started.[444]

Scheffler follows Nelson Goodman[445] and Quine[446] in placing emphasis, for the purposes of mathematics and science, on reference. He writes:

> The concept of definition itself, in mathematical and scientific contexts, hinges not on synonymies but, at most, on referential equivalences.[447]

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443 In our Appendix Two, we noted that Quine realized that a referential account of meaning (our word, not his) is not sufficient. This lead him into a study of sense (our word, he would use "meaning") and this led him to his D-thesis. The D-thesis came up in a study of sense and not reference. There are these two aspect to semantics (sense and reference).

444 Scheffler, ***op. cit***., p. 57

445 Nelson Goodman, ***The Structure of Appearance*** (New York, 1951)

446 Quine. "Notes on a Theory of Reference" ***From a Logical Point of View*** pp. 130-138.

447 Scheffler. ***op. cit***., p. 57. He has a long quote from Quine here, see p. 132 in article above.

And further, he makes a point that will be the basis of the escape from the paradoxes?

> As for deduction within scientific systems, it should be especially noted that it requires stability of meaning only in the sense of stability of reference in order to proceed without mishap.[448]

This must not be taken as saying that a referential account of meaning is a sufficient account of meaning. All that it says is that the stability of meaning that is necessary for the scientific process to take place is found at the referential level.

The traditional example of 'Evening Star' and 'Morning Star' (see our Chapter Two) is used to show that reference cannot provide a sufficient account of meaning. This example also indicates how referential meaning can provide the basis for comparison of competing theories, and the basis for communication among men holding different theories. They are an example of a situation where we have two expressions with quite different senses, but which have the same reference. Scheffler points out that "common reference may not only survive alterations of belief outside the realm of synonymies and variations affecting the synonymies of neighbouring terms within a given language system; it may also survive synonymy alterations bearing directly on the very terms in question."[449]

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448 ***Ibid.***, p. 58.

449 ***Ibid.***, p. 60.

> "Terms may denote the very same things though their synonymy relations are catalogued differently. Hence common reference may not only survive alterations of belief outside the realm of synonymies and variations affecting the synonymies of neighbouring terms within a given language system; it may also survive synonymy alterations bearing directly on the very terms in question. Opposing

According to Scheffler, theorists can disagree at the level of sense while agreeing at the level of reference. Commonality of reference removes the subjectivists' paradoxes; for it is now possible to share meanings among alternative belief systems. Differing theories can be compared through common reference.

Men must agree in order that they can disagree. In order to establish a basis for the comparison of theories, men must agree on particular cases. The possibility of agreement on cases arises from the fact that referential identities can be retained throughout variations of theoretical context.[450]

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theorists may differ in respect of these latter alterations, or may reject the idea of specifying synonymy relations altogether, and they may yet mean, that is, refer to, the same things. Their differing beliefs will then represent conflicting systems of assertions expressible with the same language apparatus, the latter individuated simply by its logic and its referential properties. Even, moreover, if their languages differ as wholes, they may well overlap with respect to certain terms, to which the assign identical referential functions, and by means of which their opposing theories may be expressed." (***Ibid***.)

450 ***Ibid***., p. 62.

"Alterations of theoretical framework, though they be understood to effect changes in relevant substantive premisses, in official definitions, and even in the senses of constituent terms, need not also be taken thereby to alter the referential meaning-identity of critical experimental formulations. Such identity may, of course, be deliberately changed for independent reasons. But the mere fact of absorption into varying frameworks of theory does not, in itself, require us to say that the old laws have altogether changed, giving way to new. The constancy of referential interpretation is, moreover, accessible to reinforcement through shared processes of agreement on particular cases; I suggest that such accessibility is indeed, the core of the idea that experimental laws are couched in a relatively independent observational language." (***Ibid***.)

The referential language is fixed by men's agreement on particular cases. One should not interpret this as implying that a referential language is fixed for all time in some ultimate sense.[451] The reference language can change, but it is better to keep it relatively stable. Languages can and do evolve with the consequent effect on the referential part, but often with natural languages this evolution is slow enough as to have a minimal effect on communication. The emphasis is on men's agreement, for men could choose to alter the particular cases that determine the basis for communication. Scheffler emphasizes that it is the possibility of agreement that is created by the indifference of reference to theoretical changes that removes us from the subjectivist bind. He writes: "the possibility of shared processes of decision on the referential force of a term by application to cases"[452] is what "allows for an independent understanding of the referential interpretation of a law by opposing theorists, and is thus sufficient to prevent the subjectivist denouement."[453]

Scheffler's solution also seems to account for the resistance that meets fairly new and innovative theories. Such theories may run up against the settled agreement on cases and as a result

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451 ***Ibid***., pp. 61-65.

"It is, however, compatible with this possibility to allow that what is judged decidable by application to cases may well vary with history, purpose, and prior theoretical context. Such variation is an index of the fact that referential modes are not forever fixed, that the learning of new languages of reference is possible. The relative independence of observation from theory must not be taken to imply that there is some single descriptive language, fixed for all time, within which science must forever fit its experimental accounts of nature... It is open to us to acknowledge a multiplicity of schemes of reference, shared in part or in whole, overlapping in lesser or greater degree, and suitable in varying ways for different purposes, yet allowing, within their several ranges, for the growth of experimental knowledge in a controlled manner."(***Ibid***.)

452 ***Ibid***., p. 64.

453 ***Ibid***.

it may be difficult to communicate such theories to the community of scholars working within the settled paradigm. As Scheffler puts it:

> A new theory arising within a given referential tradition cannot command initial consensus on presumably confirming cases of its own, but must prove itself against the background of prior judgments of particulars. It must acknowledge the indirect control of accumulated laws and theories encompassing already crystallized judgments of cases. Even if some such judgments are to be challenged from the start, the challenge needs to be expressed in a form that is intelligible for the received descriptive mode, and special motivation for the challenge must, of course, be adduced... Now, it may turn out that this new theory, having won an initial place largely through indirect forms of argument the background of acknowledged facts, eventually forces a revision of older judgments of cases, and, what is more significant, perhaps, opens up new ranges of evidential description, thereafter developing consensus on relevant instances of its own.[454]

Scheffler places emphasis on the "indirect control of accumulated laws and theories encompassing already crystallized judgments of cases." Scheffler considers such a control to be "a characteristic mark of science."[455] It is just this control that comes under examination when we consider the question of how a scientist determines what to preserve in the face of recalcitrant experience.

In one sense, science can be viewed as progressing through a dynamic process of give and take. To oversimplify matters, we can say that science oscillates between periods of settlement on particular cases (normal science) and periods of argument for consideration of different cases to be used as a basis (extraordinary science). Science takes on its institutional form in order to regulate this process.

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[454] ***Ibid***., pp. 64-65.

[455] ***Ibid***., p. 66.

Quine's D-thesis and Meaning Change:

The paradoxes of meaning change arise for the D-theoretic position if one treats it as issuing in just a context theory of meaning (as Frankfurt would have it do). Such an attitude, though, results from a failure to see that the context theory of meaning is only one aspect of the D-theoretic position. A D-theorist has a lot more to say before he considers his position close to being adequate. If we consider Quine's development up to the D-thesis, we find that prior to the "Two Dogmas..." paper (and after) he devoted considerable energies to the development of a theory of reference. In fact, it was the realization that certain important semantical notions could not be accounted for on the basis of reference (for example, synonymy, necessity, analyticity, etc.) that ultimately lead him to the D-thesis. Quine divided semantics into two distinct areas: the theory of meaning and the theory of reference. Quine's use of 'meaning' here corresponds to the Fregean use of 'sense', For Quine, 'Morning Star' and 'Evening Star' differ in meaning, but have the same reference.) So to view Quine's D-theoretic position as issuing in merely a context (connotative) theory of meaning and then criticizing him for the paradoxes (or contradictions) that arise from such a position would be like examining the motor of a refrigerator and then arguing that the refrigerator will never keep food cool let alone freeze anything because this part of the machine generates heat.

Nevertheless, the point of the paradoxes is well taken. One cannot rely upon just a context theory of meaning, otherwise one fails to have a basis for communication or a basis for the comparison of alternative theories. Something more is required. There must be some point where meanings are shared. Quine finds his shared meaning at the level of observation sentences.

Observation sentences are just those sentences that have little intersubjective variability of stimulus meaning. They "wear their meanings on their sleeves".[456] They are just those sentences "on which there is pretty sure to be firm agreement on the part of well-placed observers."[457] They are the sentences that serve as the basis for the discussion in fundamental disputes. Quine writes: "... they are just the sentences on which a scientist will tend to fall back when pressed by doubting colleagues."[458]

Thus, Quine's D-theoretic position is not subject to the paradoxes of meaning change, for agreement at the level of observation sentences provides the shared meaning necessary for communication between scientists who hold conflicting beliefs. This agreement also provides the basis for comparison, evaluation and justification of theoretical alternatives.

Quine's D-theoretic stand might be described as a controlled D-thesis. One can imagine an uncontrolled D-thesis leading to an extreme subjectivist position, subject, then, to the paradoxes. Quine's pragmatism shows through just at the point of control. The D-thesis claims that any statement can be held true come what may provided one makes drastic enough changes elsewhere in one's system, **but**, of course, the D-thesis is not applied in isolation. Applied within science it is subject to the controls that institutionalized science places on man's imagination. It is just these controls that keep the scientists's feet on the ground.

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[456] Quine, ***Word and Object*** p. 42.

[457] ***Ibid***., p. 44.

[458] ***Ibid***.

Let us return briefly to the context of falsification. At the level of normal science (day-to-day science), not only is there stability at the level of observation sentences, but also the background theory is kept stable. Thus, in normal science one can talk about an hypothesis being falsified for a given theory. At the level of normal science, the logical interconnections between sentences of the system are kept constant so that individual hypotheses can be tested under particular assumptions. Crucial experiments can and do take place within a given paradigm, or for a given system. This possibility does not legislate against the D-thesis. In normal science, the choice is to preserve the 'auxiliary assumptions' and to let the hypothesis go as falsified. However, if the hypothesis is prized for some reason or other, the scientist is permitted (by the D-thesis) to loosen up the auxiliary assumptions and change the background theory so that the prized hypothesis in conjunction with the new background theory entails what is observed. The D-thesis legislates for extraordinary (revolutionary) science. The institutional nature of science demands throughout this process that there be stability at the level of observation sentences. The scientist proposing such a change in background theory must provide convincing arguments to his peers for such a change. In doing so he is restricted to talk relative to the agreed sentences at the level of observation. A serious application of the D-thesis must be argued for, and for this reason the D-theorist must limit himself to preserving the agreed observational language. A serious application of the D-thesis is not conducted in total freedom, but rather is limited by the socio-scientific milieu in which a given proponent finds himself. Now while stability is retained for the period of the argument at the level of observation, what is dropped in extraordinary science is the demand for semantical stability in the background theory (the level of sense). Grünbaum's restriction on the D-thesis is too constraining because it ignores or fails to recognize this so-called extraordinary science.

This does not mean that observation sentences are fixed for all time, rather they are fixed (by men's agreement - such agreement may be tacit for it may be imposed only through the ostensive way that men learn language within their own socio-cultural groups)[459] temporarily at a particular time and place for the purpose of communication among men at that time and place. (Since language does evolve, we can often look back and make comparisons between our language and language that was used before us.) If a given point of view prevails over the existing paradigm and the other men of the society (in the case of science, this may be the society of scientists) are convinced, then the set of agreed cases may be changed and the observation sentences that serve as the basis for comparison and control may change. (This may be a gradual change, a subtle change, that may go unnoticed within a particular generation, but may become apparent only when one compares the language of diverse generations). Thus within a particular cultural grouping there will be an observed stability at the observational level, and this serves as the basis for communication. (Perhaps, one can account for such break-downs of communication as the 'generation gap' etc. by virtue of a change in the agreed paradigms). Quine writes, with respect to stability at the level of observation:

> Moreover, the philosophical doctrine of infallibility of observation sentences is sustained under our version. For there is scope for error and dispute only insofar as the connections with experience whereby sentences are appraised are multifarious and indirect mediated through time by theory in conflicting ways; there is none insofar as verdicts to a sentence are

[459] Quine writes: "The sort of meaning that is basic to translation, and to the learning of one's own language, is necessarily empirical meaning and nothing more. A child learns his first words and sentences by hearing and using them in the presence of appropriate stimuli. These must be external stimuli, for they must act both on the child and on the speaker from whom he is learning. Language is socially inculcated and controlled; the inculcation and control turn strictly on the keying of sentences to shared stimulation. Internal factors may vary **ad libitum** without prejudice to communication as long as the keying of language to external stimuli is undisturbed." (Quine, "Epistemology Naturalized", ***Ontological Relativity***, p. 81.)

> directly keyed to present stimulation. (This immunity to error is, however, like observationality itself, for us a matter of degree).[460]

The D-theoretic point can be made picturesque by reintroducing Neurath's analogy of rebuilding a ship at sea. Our conceptual scheme changes "... bit by bit, plank by plank, though meanwhile there is nothing to carry us along but the evolving conceptual scheme itself."[461] As Quine put it in his paper "Identity, Ostension, and Hypothesis":

> We can improve our conceptual scheme, our philosophy, bit by bit while continuing to depend on it for support; but we cannot detach ourselves from it and compare it objectively with an unconceptualized reality. Hence it is meaningless, I suggest, to inquire into the absolute correctness of a conceptual scheme as a mirror of reality. Our standard for appraising basic changes of conceptual scheme must be, not a realistic standard of correspondence to reality, but a pragmatic standard. Concepts are language, and the purpose of concepts and of language is efficacy in communication and in prediction. Such is the ultimate duty of language, science and philosophy, and it is in relation to that duty that a conceptual scheme has finally to be appraised.[462]

What results from this is quite a different view of the epistemological basis for science from the one put forward by those in the tradition of the logical positivists. In the next chapter, we shall explore the significance of the D-theoretic viewpoint for various aspects of the scientific endeavour.

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[460] Quine, ***Word and Object***, p. 11.

[461] Quine, "Identity, Ostension, and Hypostasis" ***From a Logical Point of View***, p.79.

[462] ***Ibid***.

CHAPTER FIVE

The Philosophical Significance of the D-thesis

The D-thesis is basic to Quine's attack on the logical positivist's position. The D-theoretic position contrasts with the standard view of the followers of the Vienna Circle. The one-tiered D-theoretic basis contrasts with the positivists' two-tiered basis.[463] The hard and fast distinctions in kind so common to the positivists' standard view are not accepted by the D-theorist. Instead the D-theorist considers the differences between analytic and synthetic sentences, the **a priori** and the **a posteriori**, observation sentences and theoretical sentences to be differences of degree. Also characteristic of the D-theoretic epistemology is less emphasis on "correspondence to reality" and more emphasis on the pragmatic dimensions of a theory.

Since we are more concerned to display the significance of the D-thesis and because the logical positivist position has received plenty of play over the last forty years[464], we spend time

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[463] This is Sheffler's expression. See Sheffler, ***op. cit***., p. 53. He has a brief characterization of the logical positivist's two-tiered view.

[464] There are any number of sources that present a development of the views of the Logical Positivists. Besides the writings of the active members of the Vienna Circle such as Feigl and Carnap, etc., one is directed to secondary works like:

(a) P. Achinstein & S. Barker, ***The Legacy of Logical Positivism*** (Baltimore, 1969).

(b) A.J. Ayer, ***Language, Truth, & Logic***, London, 1936; 2nd. ed., rev. 1946)

(c) A.J. Ayer, ***Logical Positivism***, (New York, 1959).

(d) G. Bergmann, ***The Metaphysics of Logical Positivism***, 2nd. ed. (Madison, 1967).

(e) C.G. Hempel, ***Aspects of Scientific Explanation***, (New York, 1965).

displaying features of the D-theoretic view in the hope that the reader will find it a plausible alternative.

In "Two Dogmas...", Quine concluded that a boundary had not yet been drawn between analytic and synthetic statements and that it was an unempirical dogma that such a distinction could be drawn at all. The D-thesis serves these conclusions since it holds that factors beyond the statements themselves determine their analyticity or syntheticity. From the D-theoretic viewpoint, truth come what may cannot decide between any statements. Such a criterion cannot distinguish between analytic and synthetic statements, for any statement (even statements that we regard as being very close to experience) can be held true come what may if one makes drastic enough alterations elsewhere in one's system of statements. Truth come what may is relative to one's choice of system. On the other side, there are no statements that are immune (- in some ultimate fashion-) from revision as so-called analytic statements are considered to be. Acceptance of the D-thesis, therefore, legislates against the bifurcation of statements into those that are analytic and those that are synthetic.[465]

(f) J. Jorgenson, ***The Development of Logical Empiricism***, (Chicago, 1951).

(g) C.W. Morris, ***Logical Positivism, Pragmatism, and Scientific Empiricism***, (Paris, 1937).

(h) E. Nagel, ***The Structure of Science***, (New York, 1961).

(i) J.R. Weinberg, ***An Examination of Logical Positivism***, (New York, 1936).

[465] Quine discusses inter-connected concepts at several places in his writings. Synonymy and analyticity are closely inter-connected.

Statements are synonymous if the biconditional ('if and only if') which joins them is analytic; names are synonymous if the statement of identity which joins them is analytic; and predicates are synonymous if, when they are applied to like variables and then combined into a universally quantified biconditional, the result is analytic.

(W.V.O. Quine, "The Problem of Interpreting Modal Logic", ***Journal of Symbolic Logic***, XII, no. 2. (June, 1947) p. 44.)

In this case synonymy is construed as being dependent upon analyticity. The relationship can be construed as going the other way.

> ... a statement is **analytic** if by putting synonyms for synonyms (e.g. 'man not married' for 'bachelor') it can be turned into a logical truth. (***Ibid***.)

or

> Given the notion of synonymity,(sic.) given also the general notion of truth, and given finally the notion of logical form (perhaps by an enumeration of the logical vocabulary), we can define an analytic statement as any statement which, by putting synonyms for synonyms, is convertible into an instance of a logical form all of whose instances are true.

(W.V.0. Quine, "Notes on Existence and Necessity", ***The Journal of Philosophy***, XL, no. 5., (March 4, 1943) p. 120.)

> Intimately tied to the notion of synonymy is the notion of meaning.
>
> Just what the **meaning** of an expression is - what kind of object - is not yet clear; but it is clear that, given a notion of meaning, we can explain the notion of **synonymity** (sic.) easily as the relation between expression that have the same meaning. Conversely also, given the relation of synonymity (sic.) it would be easy to derive the notion of meaning in the following way: the meaning of an expression is the class of all expressions synonymous with it. No doubt this second direction of construction is the more promising one. (***Ibid***.)

Meaning and analyticity are often related. "It is usual to describe an analytic statement as a statement that is true by virtue of the **meanings** of the words; or as a statement that follows logically from the meanings of the words." (***Ibid***.)

The notion of necessity also ties in as well, for it can be defined in terms of analyticity,

> the result of applying 'necessarily' to a statement is true if, and only if, the original statement is analytic.(***Ibid***.,p. 121.)

Along with necessity come the notions of 'possibly' and 'it is impossible that' which are definable in terms of necessity ('possibly' = df. 'not necessarily not'; 'it is impossible that'= df. 'necessarily not').

Quine relates confirmation and verification as well:

> As an empiricist I consider that the cognitive synonymy of statements should consist in sameness of the empirical conditions of their confirmation. A statement is analytic when its operational condition of verification is, so to speak, the null condition.

As Quine sees it, the error of the logical positivists has been in trying to find the unit of empirical significance in statements. Such a unit is smaller than can be warranted for meaning is a broader issue. Quine's suggestion is that we adopt a broader unit of empirical significance - total

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(W.V.0. Quine, "Semantics and Abstract Objects", ***Proceedings of the American Academy of Arts and Sciences***. (1951) p. 92.)

This latter relationship played a prominent role in the breakdown of the analytic-synthetic distinction.

Quine's argument is "Two Dogmas..." might be construed as follows. First, according to the D-thesis, "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body." (Quine, "Two Dogmas..." ***op. cit***., p. 41.) Thus, it is not the case "that each statement taken in isolation from its fellows, can admit of confirmation or infirmation at all." (***Ibid***.) Now if this is true then one cannot define statement synonymy which is "the likeness of method of empirical confirmation or infirmation." (***Ibid***., p. 38.) Now since we cannot define, synonymy, it follows that we cannot define analyticity which is defined in terms of synonymy. Thus, if the D-thesis is accepted the logical positivist's dogma of reduction falls, for "the dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows can admit of confirmation or infirmation at all." (***Ibid***., p. 41.)

This is precisely what the D-thesis rejects. In addition, the dogma of the cleavage between analytic and synthetic falls with it. Quine states the relation between the two dogmas as follows:

> More directly, the one dogma supports the other in this way: as long as it is taken to be significant in general to speak of the confirmation and infirmation of a statement, it seems significant to speak also of a limiting kind of statement which is vacuously confirmed, **ipso facto**, come what may; and such a statement is analytic. (***Ibid***.)

theories (systems of statements). Quine admits that science does have "its double dependence upon language and experience."[466] This was something that the logical positivists tried to capture through their distinction between analytic and synthetic statements. But Quine points out that this duality affects the whole system and "is not significantly traceable into the statements of science taken one by one."[467] This would explain why the logical positivists have difficulty in pinning down meaning.

Observation and Theory:

Let us start to build up the D-theoretic picture of science at the point where we left off in our last chapter, - that is, with the matter of observation. In Quine's theory there are no purely empirical statements (- synthetic statements). Observation statements are theory-laden (and so are all other statements). The logical positivists believed that there were certain sentences (-Carnap called them protocol sentences-) that were free of any interpretation and were directly connected to experience. These sentences were theory-free and hence theory-neutral. They were somehow construed to be purely empirical. They were entailed by but did not entail the theoretical sentences of the various theories. The theoretical sentences had no directly testable consequences, but could only be tested indirectly through their connections with the theory neutral sentences. Presumably, advocates of competing theories could compare their alternatives from the vantage point of the theory-neutral observation language.

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466 ***Ibid***., p. 42.

467 ***Ibid***.

Quine rejects such a picture of science. In view of arguments such as those presented by Hanson in his book, ***Patterns of Discovery***[468], it is hard to conceive what such a theory-neutral language could be like. Quine wrote:

> Around 1932 there was debate in the Vienna Circle over what to count as observation sentences, or **Protokollsätze**. One position was that they had the form of reports of sense impressions. Another was that they were statements of an elementary sort about the external world, e.g., "A red cube is standing on the table." Another, Neurath's, was that they had the form of reports of relations between percipients and external things: "Otto now sees a red cube on the table." The worst of it was that there seemed to be no objective way of settling the matter: no way of making real sense of the question.[469]

As we have already noted, Quine's view does not exclude observation sentences. It does, though, legislate against the view that observation sentences are somehow theory-neutral. In Quine's view, observation sentences "are sentences which, as we learn language, are most strongly conditioned to concurrent sensory stimulation rather than to stored collateral information."[470] An

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468 N.R. Hanson, ***Patterns of Discovery***, (Cambridge, 1969). See especially his early chapters. For example, Hanson presents a diagram of an X-ray tube. He tells us that a trained physicist would see the diagram as an X-ray tube viewed from the cathode. He writes:

> Would Sir Lawrence Bragg and an Eskimo baby see the same thing when looking at an X-ray tube? Yes, and no. Yes - they are visually aware of the same object. No - the **ways** in which they are visually aware are profoundly different. Seeing is not only having of a visual experience; it is also the way in which the visual experience is had.
>
> At school the physicist had gazed at this glass-and-metal instrument. Returning now, after years in University and research, his eye lights upon the same object again. Does he see the same thing now as he did then? Now he sees the instrument in terms of electrical circuit theory, thermodynamic theory, the theories of metal and glass structure, thermionic emission, optical transmission, refraction, diffraction, atomic theory, quantum theory and special relativity. (Hanson, pp. 15-16.)

469 W.V.0. Quine, "Epistemology Naturalized" ***Ontological Relativity & Other Essays***, (New York, 1969), p. 85.

470 ***Ibid***.

observation sentence is a sentence that is directly conditioned to sensory stimulation. If one is asked to assent to or to dissent from such a sentence, the verdict "depends only on the sensory stimulation present at the time."[471] This is the closest that we can get to a "purely empirical sentence". We cannot reach the ideal that the logical positivist view requires. Some stored information is required for our "empirical" sentences. Quine writes:

> The very fact of our having learned the language evinces much storing of information, and of information without which we should be in no position to give verdicts on sentences however observational.[472]

In the light of this Quine revises his definition of observation sentence to read:

> ... a sentence is an observation sentence if all verdicts on it depend on present sensory stimulation and on no stored information beyond what goes into understanding the sentence.[473]

This definition seems to make a distinction between the information that goes into understanding a sentence (mere meaning) and the information that goes beyond mere understanding. Such a view does not, however, result in a bifurcation into analytic truth (truth which issues from the meanings of words) and synthetic truth (truth which goes beyond mere meaning). Quine grants that there would be sentences that would be true by virtue of the meanings of words and that if the sentence in question

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[471] ***Ibid***.

[472] ***Ibid***., pp. 85-86.

[473] ***Ibid***., p. 86.

is simple enough it would be "subscribed to by all fluent speakers in the community."[474] But this community-wide acceptance cannot serve to distinguish analytic truths from synthetic ones, because there are a number of so-called synthetic sentences such as "There have been black dogs" that are also accepted on a community-wide basis. Quine writes:

> My rejection of the analyticity notion just means drawing no line between what goes into the mere understanding of the sentences of a language and what else the community sees eye-to-eye on. I doubt that an objective distinction can be made between meaning and such collateral information as is community-wide.[475]

We saw in our last chapter that observation sentences provide the basis for communication. (Within a given linguistic community) we agree that certain sentences are correlated with certain experiences. The learning of such correlations is part of learning our native language. For the purposes of discussion and comparison of theories, men have an agreed stability at the level of the observation language. In one sense, then, the most empirical sentences are held true come what may. That is, sentences like "this is a red ball" or just "Red ball" that are used when we teach children to use language are fixed and generally will remain correlated to the same sort of sensory experience. When saving a particular hypothesis, however, we may plead illusion, but we do not usually give up these basic correlations. In a sense, then, it is our most empirical sentences and not the so-called analytic sentences (of logic, mathematics, etc.) that are given up last. This seems to run counter to Quine's view in "Two Dogmas..." that "our tendency to disturb the total system as little as possible

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[474] ***Ibid***.

[475] ***Ibid***.

would lead us to focus our revisions upon these specific statements concerning brick houses and centaurs"[476] rather than on the "highly theoretical statements of physics or logic or ontology."[477]

This is one place where I think Quine's metaphor tends to mislead. Throughout the history of science probably very little has been done to alter our simple kinds of observation sentences like, "This is green", "This is a finger", and "That is a ball", that we use to introduce our children to language; whereas, a great deal of alteration has taken place in statements of physics, logic and ontology (metaphysics). We no longer speak of phlogiston, ether, absolutes and so on. Logic, for example, has changed drastically in just this last century. One also has only to think of the changes that have taken place in the language of physics since the development of special and general theories of relativity, quantum mechanics, solid state physics and the like. It is the highly theoretical statements that are more subject to change; - those sentences that are at the "centre" of our system. Our most empirical sentences, those at the periphery, are the ones less liable to change. In another sense, though, it is the theoretical ones that are given up last. In order to save a prized theory we are willing to plead illusion rather than give up the theory. Thus, for a given theory, the theoretical ones are the last to change; because if they are changed we have a different theory.

This correction in Quine's metaphor does not do harm to his field theory of knowledge, but rather brings it more in line with what actually happens. As we discussed in our Chapter Three, men must agree in order to disagree. Men agree at the level of their observation language (reference

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476 Quine, "Two Dogmas..." ***op. cit***., p. 44.

477 ***Ibid***.

language] in order that they may disagree at the level of their theoretical language (sense language). Men agree on their observation language so that they may discuss, compare and evaluate their alternative theories. Such discussion, comparison, and evaluation, may lead to an alteration of the agreed observation language, but this occurs, perhaps, only after several generations of subtle work.

At the level of observation, then, there is an agreed stability in our "most empirical" sentences. At the level of theories, however, Quine's metaphor may indeed follow through. A change in the highly theoretical sentences of a given subject will result in a change of theory. Thus, there is a sense in which they are given up last. They are given up last with respect to a given theory. If they are given up, then the theory is given up (-changed, or altered).

From the D-theoretic stand-point, then, there is no hard and fast distinction between observation statements and theoretical statements. The difference is one of degree. Observation sentences while theory-laden are theory-laden only to the extent that a certain amount of collateral information is needed in order to understand a language. (We must be able to correlate its names with the objects of the world that are being referred to, its predicates with qualities and relations and so on.) The understanding of theoretical sentences goes beyond that required for the mere understanding of language. A great deal of collateral information is presupposed. In order to understand the highly theoretical sentences of physics, one needs to have studied a considerable amount of physics (-collateral information not available to the average person in a community).

Within a special discipline such as physics the set of observation sentences may be expanded considerably from those found in the community at large. Quine writes:

> For by our definition the observation sentences are the sentences on which all members of the community will agree under uniform stimulation. And what is the criterion of membership in the same community? Simply general fluency of dialogue. This criterion admits of degrees, and indeed we may usefully take the community more narrowly for some studies than for others. What count as observation sentences for a community of specialists would not always so count for a larger community.[478]

Observation, thus, varies according to the "'width of the community considered."[479] This accounts for the difference, discussed by Hanson[480], between the response of a physicist when observing an X-ray tube and that of an average man. "One man's observation is another man's closed book or flight of fancy."[481]

Quine writes:

> The observation sentence is the cornerstone of semantics. For it is ... fundamental to the learning of meaning. Also, it is where meaning is firmest. Sentences higher up in theories have no empirical consequences they can call their own; they confront the tribunal of sensory evidence only in more or less aggregates. The observation sentences, situated at the sensory periphery of the body scientific, is the minimal verifiable aggregate; it has an empirical content all its own and wears it on its sleeve.[482]

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478 Quine, "Epistemology...", ***op. cit***., p. 87.

479 ***Ibid***., p. 88.

480 See example in footnote 6 (above).

481 ***Ibid***., p. 88.

482 ***Ibid***. p. 89.

This section briefly summarizes the difference between observation and theory. There is no pure perception needed in this account. Percepts come replete with concepts. Observation is not theory-free. Experience must come conceptualized; and observation must come theory laden.

The Logic of Hypothesis (Falsification):

The lesson of the D-thesis is more apparent in the context of hypothesis testing. Individual hypotheses are not separately falsifiable. The logical form of falsification is not represented by the simple modus tollens argument form such as:

$$\begin{array}{c} H \rightarrow O \\ \sim O \\ \hline \sim H \end{array}$$

where H is an individual hypothesis, O is an observation sentence. This, as we have seen, is an over-simplified view of falsification. The more complex Duhemian picture was represented as follows:

$$\begin{array}{c} (H \,\&\, A) \rightarrow O \\ \sim O \\ \hline \sim (H \,\&\, A) \end{array}$$

where H is an individual hypothesis, O is an observational sentence, and A represents the set of auxiliary assumptions (A_1, A_2, ... , A_n), ($n > 1$). If we substitute for A, then one can easily see that H is not separately falsifiable. That is, the experiment shows: $\sim (H \& A_1 \& A_2 \& ... \& A_n)$. Using DeMorgan's principle, it shows that one cannot be clear about where to place the blame for the false prediction. That is, one has:

$$\sim H \ \text{v} \sim A_1 \ \text{v} \sim A_2 \ \text{v} \ ... \ \text{v} \sim A_n.$$

The falsity may be attributable to one or more of the disjuncts. The experiment does not designate which of the disjuncts (or set of disjuncts) is to blame for the derivation of the false experimental prediction.

You will recall that Quine's D-theoretic view went one step beyond the Duhemian claim. In addition to the Duhemian claim that no individual hypothesis is separately falsifiable, Quine claimed that we can choose to retain the hypothesis by placing the blame on one or more of the A_1, A_2 ..., A_n and then making the appropriate revision so that the hypothesis yields the observation that did obtain. The question that arises from this additional claim is: How do we decide which revision is the appropriate revision? This question, of course, presupposes a positive response to the query: Are there other revisions possible besides rejecting the hypothesis?[483]

483 Quine's qualification "drastic enough adjustments elsewhere in the system" certainly is strong enough to enable us to find a very large number of alternatives. It leaves the process of revision fairly open.

It is just at this point where Quine's account becomes vague. (Most other accounts seem also to be vague as well). The vagueness is most likely attributable to the fact that matters become quite complex at this point and as a result it is very difficult to get a hold on the matter. One cannot just say that "good sense" tells the scientist where to make his alterations. This is not good enough. To say that scientists employ a strategy of "minimum mutilation"[484] does not provide a sufficient account. (It merely raises the further question to how the maxim of minimum mutilation operates.[485] It does not do to merely suggest that when we decide to "adjust one strand of the fabric of science rather than another in accommodating some particular recalcitrant experience"[486], that conservatism figures such choices, and so does the quest for simplicity."[487] What is needed is some further account of conservatism and simplicity.

We can take a clue, however, from one of Quine's critics, C.K. Herburt. In his paper entitled, "The Analytic and the Synthetic"[488] he writes:

> ... the testing situation as described by the Duhemian argument, is never as hopeless in practice as it may seem in theory. It is true we always test a system of hypotheses or a system

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[484] W.V.0. Quine, ***The Philosophy of Logic***, (Englewood Cliffs, 1970).

[485] Just as Quine criticized the notion of 'the meaning' as providing an explanation in terms of some mystical entity that needed also to be explained, (Likewise, Hume on substance) so too we can say that the phrase "maxim of minimum mutilation" does very little in the way of explanation unless it is itself made explicit.

[486] Quine, "Two Dogmas...", p. 46.

[487] ***Ibid***.

[488] G.K. Herburt, "***The Analytic and the Synthetic",Philosophy of Science***, XXVI, (1959), pp. 104-113.

> of statements, and in the case of falsification we are faced with a choice as to which part of our system should be amended. Still, we have reasons (although not logically conclusive reasons) to consider some parts of our system better established than other parts. If that were not so, we could not make even the first step in scientific inquiry.[489]

Herburt points to the fact that a bespectacled scientist relies on certain laws of optics in making his observations. Naturally, he does not bring these into question when performing his research. (We presume, of course, that he is not doing research on optics, that is, optics as they specifically relate to his corrective lens. We are talking of the scientist in general who could be doing work in biology, geography, etc. as well as physics). Herburt points out that if our scientist did doubt the laws of optics, "he would have to take the glasses off every time he looks at his instruments."[490] He adds: "Of course, he considers the scientific hypotheses according to which his glasses were made better established than the hypotheses under test."[491]

Herburt's point is well-taken. Scientists certainly do not in practice place the totality of all their knowledge and beliefs on the examination table. (Most of them, probably, could not even give a complete statement of all their knowledge and beliefs). As Herburt recognizes, the Duhemian point is a logical point. While it is true that in normal scientific contexts tests are directed at particular hypotheses, logically not only these hypotheses but also all the auxiliary hypotheses are under test.

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489 ***Ibid***. p. 111.

490 ***Ibid***.

491 ***Ibid***.

Men choose, though, to restrict the tests by presupposing certain things to be true for the purposes of the test. Arne Naess puts the point as follows:

> ... when a scientist, in his actual work, takes something for granted, without any reservations whatsoever, this does not imply that he holds certain assumptions, principles, or laws to be true or highly confirmed. He is at the moment not concerned with the truth or certainty of **those** propositions. Research practice requires limitation of perspective during each piece of work.[492]

> Herburt's characterization of Quine's view goes as follows:
>
> In Quine's view our testing situation would look approximately like this:
>
> $$(L . M . E) \supset q$$
>
> where L stands for our logic, M-for mathematics, E-for a system of empirical hypotheses, laws, initial conditions, etc.[493]

He, then, poses several questions as they relate to a confirmation situation and a falsification situation:

> In the case of confirmation we find that q. Can we say, then that we have **confirmed** L and M together with E? Can we say that our mathematics and logic is valid **because** it is confirmed in experience in the same way as E? Did we not know **before** the test that they are valid, or rather that their validity was assumed for the purposes of the test?
>
> In the case of falsification we find that ~q. If so, we can adjust either L or M or E or parts of them, or any combination of them. It is true that we can "save the appearances" in any of

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492 A. Naess, ***The Pluralist and the Possibilist Aspect of the Scientific Enterprise***, (Oslo, 1972) p. 7 .

493 Herburt, "The Analytic..." ***op. cit***., p. 112.

Herburt uses '? ', the symbol for material implication. As we have stated elsewhere in the thesis, we do not consider this to be an adequate entailment for these purposes. We will ignore this use of material implication here for its use does not really hinder the point.

> these ways. But does it mean that we have submitted our L and our M to the test in the same way as our E? And suppose that we decided to revise our M and, for example, use Riemannian geometry instead of Euclidean geometry. Does it mean that Euclidean geometry has become false because it is not used for the purpose of explanation? Doubts expressed in this series of questions indicate, it seems to me, some of the consequences of Quine's position and these consequences are difficult to admit.[494]

Given what has been said in our dissertation, the answers to Herburt's questions are obvious. A finding, q, is not only a confirmation of a given empirical hypothesis with its supporting laws and initial conditions, but it also confirms the logical and mathematical bases for the system in which the test hypothesis(es) is (are) couched. ('Confirms', of course, is used in the sense of strengthen.) Clearly, q provides another confirming instance (-still a long way from conclusive verification). If the mathematics and logic was formally valid before the test, they, of course, remain so after the test (even if the result is positive or negative). The confirmation provides further support for the selection of the particular logical and mathematical system. There may possibly be a better basis than the one chosen, but this particular experiment provides no reason for changing to one of the alternative valid bases. However, in the case of a finding, ~q, the experimenter is faced with a choice. He could decide to employ a different mathematical basis (e.g. use a Riemannian geometry instead of Euclidean geometry). Should he choose to make a change, it does not affect the formal status of the old mathematical basis, but only indicates that the experimenter has found what he feels is a better basis for his explanation.

In normal (everyday) scientific settings, scientists do assume the validity of their particular logic and mathematics. This does not mean that they cannot be replaced, but rather it means that for

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[494] ***Ibid***.

the purposes of everyday science they do not come into question. Usually they are firmly entrenched because the experience of many years work has shown these parts of knowledge to be fairly reliable. The choice, of course, is pragmatic.

In the case of confirmation, a successful prediction of q not only adds strength , the individual hypotheses being tested, but also further reinforces the logico-mathematical milieu in which the test was constructed and in terms of which the experiment was couched. While one cannot know in some absolute sense that L and M are valid, one can rightfully claim their validity on the basis of their being firmly entrenched in the scientific context in which the test is being conducted.

In the case of falsification, blame is usually attached to the hypothesis being tested. Basically, it is chosen simply because it is not as firmly entrenched as the logico-mathematical background. Quine's point is that while we normally do not choose to alter the background theory ("our natural tendency is to disturb the total system as little as possible"[495]), there is nothing that logically forbids us from doing so, and, in fact, should we decide to "save" the hypothesis (for some reason or other) we could do so by altering the background theory in some way. As we have already indicated, pragmatically the scientist does not treat the hypothesis being tested in the same way as the auxiliary assumptions, however, logically there is no reason why such should be the case.

Herburt's example of changing a mathematical part of the background theory by the replacement of Euclidean geometry by Riemannian geometry raises the question: "Does it mean that

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[495] Quine, "Two Dogmas...", p. 44.

Euclidean geometry has become false because it is not used for the purposes of explanation?"[496] This, however, is a misleading question. Since truth or falsity is usually attached to interpreted systems of geometry the question would be better expressed in terms of an Euclidean description being replaced by a Riemannian description. In this sense, then, the Euclidean description would have to be considered in this context of explanation as not a true description, whereas the explanatory context treats the Riemannian description as true.

Herburt's criticisms do show the situation to be quite complex involving much more than vague pronouncements. To return to our symbolization of the test situation, we need to fill out the nature of our auxiliary assumptions $A_1, A_2, \ldots, A_n$. Herburt's suggestion is that not only are empirical laws and initial conditions contained in this set, but also statements of our logic and mathematics. To go a step further, this set contains also our metaphysics (ontology) and our values (axiology). In Quine's words it contains "the totality of our so-called knowledge or beliefs."[497]

We should point out, again, that, even though all one's beliefs (as they relate to the context of the test) are being tested in a test situation, for pragmatic reasons, we tend to "disturb the total system as little as possible".[498] To indicate this tendency, we have talked in terms of normal science as against abnormal science. This, however, should not be taken as indicative of some distinction between kinds of science, but rather it is intended as indicative of a difference of degree among

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[496] Herburt, p. 112.

[497] Quine, "Two Dogmas...", p. 42.

[498] ***Ibid***., p. 44.

various scientific procedures. In a normal situation, all or most of the A_i in our $A_1, A_2, ..., A_n$ are kept stable. (The hypothesis is usually given up in the case of disconfirmation). The situation is considered less and less normal (or more and more abnormal) depending on how much of the auxiliary set we permit to be challenged in the test situation. The most extreme case of abnormal science would be the case where we abandon all stability of belief (perhaps a form of insanity), but in this case we run the risk of failure to communicate with our co-workers. Logically, then, all our beliefs are continually subject to confirmation and disconfirmation, but pragmatically only a small number (usually only one) of our beliefs is permitted to be tested in an experimental situation. We are still left with the question of how we decide what to preserve and what to alter? What are the pragmatic dimensions?

Let us summarize the situation of hypothesis testing that has been presented above. In a situation where a hypothesis is being tested in normal science, the background is stabilized, and in such a situation, the hypothesis is retained or let go depending on whether the predictions based upon the hypothesis obtain or fail to obtain. In order for confirmation or disconfirmation to take place, the observations must be couched in terms of a particular theory. The context of experiment must be stabilized so that the falsification or verification can carry back to the hypothesis and its auxiliary assumptions. In order for falsification to take place there must be the possibility of constructing a contradiction. In order for contradiction to occur, there must be a sharing of meaning and so the experimental context must be stabilized within a particular background or milieu. The next stage of the process is the stage of decision. This is where one decides whether to change one's background in order to preserve an hypothesis in the face of disconfirmation.

The process of testing an hypothesis takes place in a condition of stability (sharing of terms). Normal science differs from abnormal science according to the decision taken regarding what is to be preserved in the face of disconfirmation. Usually if the predicted observation obtains, then the hypothesis and background receive further confirmation and one moves on to the next test. Here there is no reason to talk of normal and abnormal science. In the case of a disconfirmation or failure of prediction, one must decide where to place the blame. In most cases, where the background is reasonably settled the hypothesis goes as falsified. In some cases (cases of abnormal science) one may prefer to save the hypothesis and this entails letting some part of the background go. In such a situation, then, a new theory is developed and thereafter a new test situation. In the new test situation, there may or may not be a similarity of terms with those used in the old test situation depending, of course, on how one altered the original background.

Crucial Experiments:

One often finds a situation where there are two competing hypotheses H_1 and H_2. On the basis of evidence found prior to the experiment either hypothesis seems to be equally good. A crucial experiment is constructed as a means of deciding between the two alternatives.

In such an experiment an experimental set-up is arranged in which we have H_1 entailing an observational outcome, O_1, that is incompatible with (or conflicts with) the observational outcome, O_2, which is predicted (entailed) by hypothesis, H_2. In other words, an experimental situation is constructed in which H_1 and H_2 predict incompatible observation sentences. Given such a set-up, then, all that one need do is to try the experiment and see which prediction obtains. Then, one can reject the hypothesis from which the false prediction was derived. Symbolically we have:

(1) $H_1 \vee H_2$

(2) $H_1 \rightarrow O_1$

(3) $H_2 \rightarrow O_2$

(4) $\sim (O_1 \;\&\; O_2)$

Given this experimental set-up, the experiment is tried and one or the other of the observational consequences obtains.[499] Let us say that O_2 obtains. Now by DeMorgan's principle, Double Negation, and Disjunctive Syllogism we can derive:

(5) $\sim O_1$

Then using Modus Tollens (the assumption here is that the entailment being used permits some form of Modus Tollens), we get:

(6) $\sim H_1$

Then by a further step of Disjunctive Syllogism we have:

(7) H_2

[499] It could happen that neither prediction will occur, but for the purposes of describing the situation of a crucial experiment where one does in fact occur, we assume that one occurs. A case where neither prediction occurred would be something other than a crucial experiment.

Duhem (see Appendix One) objected that this argument presupposes that the disjunction (1) $H_1 \vee H_2$ is true. That is, it presupposes that it exhausts all the possible hypotheses. The trouble with this is that there might be a third hypothesis, H_3 which entails the observational findings and which is better than H_2, or there might even be some further hypothesis, H_4, which in conjunction with H entails the observational findings and is also better than H_2. Duhem's point is that the class of possible hypotheses is potentially infinite and for this reason crucial experiments (in some ultimate sense) are impossible.

One way out of Duhem's criticism is to argue that in normal science, crucial experiments are conducted in quite limited contexts so that only a finite (and small) set of alternatives is being considered. From this point of view one can consider crucial experiments to be effective relative to that limited context. They are crucial in a relative way.

A second objection (-not used by Duhem, but derivable from other of Duhem's views - See Appendix One) denies that individual hypotheses by themselves have observational consequences. Hypotheses are not testable in isolation. On this view, we could never have:

(2) $H_1 \rightarrow O_1$ and,

(3) $H_2 \rightarrow O_2$,

and, therefore, the argument (above) would not go through as presented. (This objection, of course, follows from the D-theoretic point that no individual hypothesis is separately verifiable or falsifiable.)

Now in normal science, crucial experiments can take place in spite of this objection. They take place within contexts where the auxiliary assumptions are assumed to be stable. On such a view, we have:

(2') $(H_1 \ \& \ A) \rightarrow O_1$

(3') $(H_2 \ \& \ A) \rightarrow O_2$

where the attendant assumptions, A, are assumed to be true for the purposes of the experiment. Thus, in the context of a stable A, the falsity of prediction can be blamed on H_1.

Crucial experiments can, therefore, take place in normal scientific endeavours if two assumptions are made:

(a) The experiment is conducted within a limited context (that is, it is restricted to a finite set of alternatives) and is so qualified.

(b) The stability of the background assumptions is assumed and the experiment is specified as being crucial relative to this background.

Crucial experiments are, thus, crucial only within a limited context and not in some ultimate sense. A given hypothesis cannot be condemned for all time on the basis of a 'crucial' experiment. Some hypotheses can be condemned within some scientific settings, but such a condemned hypothesis may well serve as a viable hypothesis in another scientific setting.

The Linguistic Component in Science

Some scientists[500] tend to ignore the linguistic component in their pronouncements. They tend to view language as somehow transparent and theory-neutral. Language is treated as a mere tool to be used in the process of determining what is or what is not the case about the world. The tool has very little if anything to do with the way the world is. A ball either has the colour red or it does not have the colour red. Language has nothing to do with the matter. It is simply our means of stating something about the matter. We can state the matter in any number of ways; - we could state it using English, or German, or Japanese. The fact remains unaffected by our choice of language.

Most scientists and, probably, most other people, would tend to label the saving of the statement, "ordinary buttermilk is poisonous" by changing the intension of 'buttermilk' to the intension of 'arsenic' as a trivial move. By making such a move, they would say, one has not changed the fact in the world. All that one has done is changed the language used to talk about the world. What is important for science is the fact expressed and not the particular language of expression. Philosophers can play their games with words and languages, they might say, but these games are not important for the working scientist. The working scientist will use the language of his social cultural milieu (e.g. English, French, Russian) plus whatever language is needed in his particular area of study (e.g. a physicist needs the language of mathematics, talk about electrons, molecules, forces, etc; a biologist needs language for taxonomic purposes, the language of biochemistry, etc.) - all of which

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500 I would like to hope that this is a small number, but, I fear that there are too many. They have not been helped by certain misleading statements made by philosophers.

has a clear sense for other members of his community. He would look askance at anyone who advocates such a trivial move as in the 'buttermilk' example.

This attitude of the working scientist and others is a healthy one. It preserves the stability not only necessary for communication, but also necessary for the criticism and evaluation of alternative theories. Certainly, such a move as envisaged by the 'buttermilk' example is trivial. It does nothing to change the facts of the matter. Such simple cases have been decided for us long ago. Very few people would spend much time trying to save the statement, "Ordinary buttermilk is poisonous".[501]

Normal science works best in such an atmosphere of linguistic stability. However, attitudes of most persons, while indicative of how they behave in certain contexts, is not necessarily indicative of the way things are. How one behaves in simple and virtually settled cases is not indicative of how one ought to behave in unsettled or controversial cases. It is in these latter cases where the non-transparency of language becomes most apparent. It is in the more difficult cases (where decisions have yet to be made) where the effect of the linguistic tool becomes apparent. It is in these cases where we have not settled on certain 'facts', that the distinctions break down. Usually we try to settle such cases in such a way as to conform to the cases already settled. Our natural tendency is "to disturb the total system as little as possible."[502] (Sometimes, however, there may be good reasons for drastic changes.)

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[501] One can, of course, devise a perverse situation where one's life might depend on saving such a statement (-say, in the setting of a concentration camp or some other perverse set-up), then one can conceive of someone diligently trying to save such a sentence.

[502] Quine, "Two Dogmas...", p. 11.

One of the roles of the philosopher and usually one of the roles of the theoretical scientist is from time to time to remind the ordinary working scientist and even the ordinary person of the linguistic component. This reminding is not only needed in science. Difficult cases arise in legal situations, in questions of morals, and so on.

This is probably why one finds the philosopher studying a variety of matters (e.g. philosophy of law, philosophy of science, ethics, philosophy of history, and so on). The philosopher functions in unsettled situations. The process of settlement is philosophic in nature. It involves the sorting out of concepts. Particularly appropriate is J. L. Austin's remark:

> In the history of human inquiry, philosophy has the place of the initial central sun, seminal and tumultuous, from time to time it throws off some portion of itself to take station as a science, a planet, cool and well regulated, progressing steadily towards a distant final state. This happened long ago at the birth of mathematics, and again at the birth of physics: only in the last century we have witnessed the same process once again, slow and at the time almost imperceptible, in the birth of the science of mathematical logic, through the Joint labours of philosophers and mathematicians.[503]

Not only does philosophy give birth to the various sciences when the unsettled becomes settled, but it continues to function in a matronly way to settle any upsets that may come upon its offspring. The crucial question is Just how should one go about the settlement of upsets?

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503 J.L. Austin, ***Philosophical Papers*** (Oxford, 1st ed., 1961; 2nd. ed., 1970) p. 180 (1st ed.). p. 232. (2nd. ed.).

Pragmatic Factors:

In the settlement of upsets the question arises: When should one alter one's science in order to preserve some favoured hypothesis in the face of conflicting evidence? How does one decide when to exercise the "natural tendency to disturb the total system as little as possible"?

There are no simple answers. Clearly, though, different techniques function at different levels of investigation. It is a very common technique for a scientist to choose to concentrate on a particular hypothesis rather than the accompanying background theory. Falsification functions as a means for deciding between alternative hypotheses within contexts where the background theory has been stabilized. Even though it may appear that a small part of science is under fire, in effect, the whole of science is under fire. Man's limited capacity dictates the use of piecemeal approach and so practical considerations of control create the need to limit investigations.

While falsification of hypotheses is a normal occurrence in science - a result of what Quine calls our natural tendency to disturb the total system as little as possible - the matter is not so simple should one decide to save a given hypothesis by placing the blame elsewhere in the system. This procedure of making changes in the background theory is not unusual in the history of science. There are the popular examples of the Copernican revolution and of Einstein's overthrow of Newtonian mechanics.

To totally dispense with one's scientific-cultural milieu would result in a breakdown in communication with the community of scientists. This communication is essential if one's theories are to have an impact, for impact presupposes that a significant part of the scientific community accepts the theory. Acceptance of a theory presupposes communication and so to have an impact a scientist must preserve some lines of communication with the community of scientists. The individual scientist cannot, therefore, reject the background assumptions of the scientific milieu in a wholesale fashion.

Pragmatic considerations enter into a scientist's decision of what to preserve and what to alter. In "On What There Is"[504], Quine suggests that our **interests** and **purposes** play a part. Every conceptual scheme carries its own ontology along with it and one judges the usefulness of such an ontological organization of experience from an epistemological viewpoint. Such a point of view "is one among various, corresponding to one among our various interests and purposes."[505]

Simplicity also figures in the scientist's choice. Quine writes:

> The edge of the system must be kept squared with experience, the rest, with all its elaborate myths or fictions, has as its objective the simplicity of laws.[506]

and further:

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504 W.V.0. Quine, "On What There Is", ***From a Logical Point of View***, pp. 1-19.

505 ***Ibid***., p. 19.

506 Quine, "Two Dogmas...", ***op. cit***., p. 15.

> Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged.[507]

The notion of simplicity is not without its difficulties. Quine admits that simplicity is "not a clear and unambiguous idea."[508] It would seem to be a matter that is relative to the aims and purposes of a given inquiry. Conceptual schemes have their own special simplicity and their own advantages. In some contexts, Quine points out, a physicalistic conceptual scheme will be simpler than a phenomenalistic conceptual scheme; whereas in other contexts it may be more cumbersome. One presumes that all that is meant is that in everyday speech talk in terms of physical objects is simpler than talk of red patches here now; whereas phenomenalists will argue that the concepts of physical objects are extremely more complex epistemologically than red patches.

Another factor mentioned is **convenience**. When one chooses an appropriate ontology it is not a matter of fact, but often rather a question of "choosing a convenient language form, a convenient conceptual scheme or framework for science."[509]

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[507] Quine, "On What..." ***op. cit***., p. 16.

[508] ***Ibid***., p. 17.

[509] Quine, "Two Dogmas...", p. 15.

Other factors are **conservatism**[510], **efficacy in communication**[511], elegance and conceptual economy[512]. The latter two are probably connected with the factor of simplicity. All of these latter three (and probably the earlier ones too) involve a psychological element - what Quine calls "psychological manageability."[513]

Often a strategy, which Quine calls the "maxim of minimum mutilation,"[514] operates. This is an extension of the claim that our natural tendency is to disturb the total system as little as possible.

> Thus suppose that from a combined dozen of our theoretical beliefs a scientist derive a prediction in molecular biology, and the prediction fails. He is apt to scrutinize for possible revision only the half dozen beliefs that belonged to molecular biology rather than tamper with the more general half dozen having to do with logic and arithmetic and the gross behaviour of bodies.[515]

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510 "Conservatism figures in such choices." ("Two Dogmas..." p. 46.)

511 "Concepts are language, and the purpose of concepts and of language is efficacy in communication and in prediction. Such is the ultimate duty of language, science, and philosophy, and it is in relation to that duty that a conceptual scheme has finally to be appraised." (Quine, "Identity, Ostension, and Hypothesis", ***From a Logical Point of View***, p. 79.)

512 "Elegance, conceptual economy, also enters as an objective. But this virtue, engaging though it is, is secondary - sometimes in one way and sometimes in another. Elegance can make the difference between a psychologically manageable conceptual scheme and one that is too unwieldy for poor minds to cope with effectively." (Quine "Identity,..." p. 79.)

513 ***Ibid***.

514 W.V.0. Quine, ***Philosophy of Logic***, (Englewood Cliffs, N.J., 1970) p. 7.

515 ***Ibid***.

One of the six beliefs from molecular biology will be selected as more suspect than the others. Conservatism comes into play in such situations, for in so-called settled cases scientists tend to focus on the feature that is not firmly entrenched in the scientific cultural milieu.

The problem is that Quine has not defined the factors precisely. The maxim of minimum mutilation, for example, leaves open the question of how one determines what changes involve a minimum of mutilation. We need to discover how the scientist knows which hypothesis to fix "upon as more tentative and suspect than other parts of the theory."[516] Many more questions can be posed about each of the factors. Clearly, though, we can say, that a number of the factors are interconnected. Conservatism, efficacy in communication, simplicity, elegance, and conceptual economy are functions of our aims and purposes. This suggests that a potential starting place would be to study the role played by purposes and interests in the process of science.

Richard Rudner, for example, has recognized that a consequence of Quine's D-theoretic stand is that the "scientist Qua scientist makes value judgements."[517] Insofar as the scientist as scientist accepts and rejects hypotheses, he must make value judgments. He says:

> For, since no scientific hypothesis is ever completely verified, in accepting a hypothesis the scientists must make the decision that the evidence is **sufficiently** strong or that the probability is **sufficiently** high to warrant the acceptance of the hypothesis. Obviously our decision regarding the evidence and respecting how strong is "strong enough", is going to be

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[516] ***Ibid***.

[517] Richard Rudner, "The Scientist **Qua** Scientist Makes Value Judgments" ***Philosophy of Science*** XX, no. 1, (Jan., 1953).

a function of the **importance**, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis.[518]

Basically what Rudner is pointing out is that certain utility considerations enter into our decision to accept or reject a given hypothesis. The cost of making a mistake must be determined. That is, in the course of the testing of scientific hypotheses a factor that enters into the decision to accept or reject an hypothesis is the cost of accepting a false hypothesis as true or rejecting a true hypothesis as false.

C.W. Churchman also holds to the view "that the theory of inference in science demands the use of ethical judgments."[519] He argues that "the determination of how ideals relative to which ends are to be weighted in inductive inference, and how the method of selecting an hypothesis is to be evaluated, is a problem of ethics".[520]

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[518] ***Ibid***., p. 2.

[519] C. West Churchman, "Statistics, Pragmatics, Induction", ***Philosophy of Science*** XV (1948) p. 265.

[520] ***Ibid***. Churchman's conclusions are dependent upon mathematical formulations of the design of experimental situations in science and the testing of hypotheses: see the work of Wald, Neyman & Pearson, Fisher and others.

Abraham Wald, ***Statistical Decision Functions*** (New York 1950, 1971)

A. Wald, ***On the Principles of Statistical Inference***. Notre Dame Math. Lectures, (1942)

Wald's book, ***Statistical Decision Functions***, has an excellent bibliography.

Neyman, J. and Pearson, E.S. "On the Problem of the Most Efficient Tests of Statistical Hypotheses," ***Phil. Trans***. Roy. Soc. of London, Ser. A. 231, (1933), 289.

R.A. Fisher, ***The Design of Experiments***, (London, 1942).

In the process of accepting or rejecting hypotheses one needs, according to Churchman, to

Quine, Rudner, and Churchman are reacting to the stifling views of the scientific enterprise that developed during the latter parts of the nineteenth and the early twentieth century. Churchman writes:

> We must abandon the nineteenth century concept of the de-personalized scientist who "merely" describes, and does not explain or moralize, about the natural world. One cannot describe without taking into account the various modes of description and selecting the "best" one. And it does no good to say that the scientists' morals are morals of "simplicity" and "convenience" ...unless we are willing to become scientific about these concepts and tell what they mean and why they are the desiderata of methodology.[521]

One can view the D-theoretic position as involving a foundational shift. Human purposes and interests become fundamental (and not sense data or intuitions.) Decisions about what to alter in the face of recalcitrant experience are taken on the basis of pragmatic considerations which determine what is useful in terms of human purposes.

It is difficult at this juncture to see how one could justify the selection of human purposes as fundamental. The fact that we are concerned with **human** knowledge, shows why human purposes are relevant rather than the purposes of some other kind of being.

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define a risk function - that is a function by means of which one can calculate the cost of being wrong (rejecting a true hypothesis or accepting a false hypothesis). C.G. Hempel and Isaac Levi reject the Churchman point that one cannot, therefore, solve questions of fact without solving value questions. Hempel and Levi attempt to develop utility functions (epistemic utilities) without the use of ethics.

[521] Churchman, ***op. cit***., p. 266.

It is a fairly common opinion that science advanced fairly rapidly following the Middle Ages when it was purged of its teleological orientation. One task of the D-theorist will be to show why it is now useful again to introduce talk of purposes. Perhaps he might argue that it will help us to organize human knowledge more effectively. However to introduce talk of effectiveness brings along with it the idea of achieving goals and objectives - and hence further talk of purposes and interests. From a broader perspective, it is difficult to see how one could argue for the fundamentality of something without appealing to itself (hence, question-begging) or to something more fundamental (and hence defeating the earlier claim of fundamentality).

The System of Science

Quine has compared science to a field of force, but, perhaps, a better image is that of an organism. Like an organism science consists of diverse but inter-connected parts. The liver and eye perform different functions and are vastly different systems, yet they are interconnected through the organism of which they are parts. Biology is quite different from Geography, yet from the D-theoretic point of view there are logical interconnections which tie these diverse subjects into a whole system of science (or knowledge).

The major point of the doctrine found in the last few pages of the "Two Dogmas..." paper was to emphasize the systematic inter-connectedness of the sciences. To use Quine's image, the setting out of the weave pattern of the fabric of science would be to make precise the logical inter-connections among the diverse parts of 'total science'.

Conclusion:

Analysis has shown the D-thesis to be an immensely complex thesis touching the fundamentals of both epistemology and ontology and not something that can be dismissed with a wave of the hand.

Philosophers of science have tended to place too much emphasis on the empirical nature of science and not enough emphasis on the role played by certain pragmatic constraints. One significant feature of the D-theoretic point of view is to draw attention to the role that these constraints play in our creation of scientific theories. If any statement can be held true in the face of recalcitrant experience by making the appropriate adjustments in one's scientific system, then an adequate analysis of scientific theorizing must take account of the factors at work when one adjustment is chosen over the other possible adjustments. It has been suggested here and by Quine that one selects one's conceptual scheme (and hence the objects of one's discourse) according to one's aims and purposes. Discrimination among equally empirical conceptual schemes is made pragmatically - the pragmatic factors being in part a function of one's aims and purposes.

The D-theoretic picture of science puts emphasis on the systematic inter-connectedness of the statements of science. Each statement finds its sense as part of a system or theory. Quine's theory suggests the need for a systems analysis of science in order to determine the inter-relations and inter-connections of the parts of science. The meaning of a sentence is a function of its logical connections with other sentences. There is no absolute distinction in kind between analytic and synthetic statements, but rather if these labels are to be attached to statements at all it is to indicate a difference in degree and then merely to indicate the relative position of a particular statement in a particular system of science. The analytic sentences can be described as those sentences which we choose to hold true in the face of most recalcitrant experience (- I use 'most' here to indicate that no sentence is immune from revision). The most synthetic sentences are those that would be given up readily as falsified in the face of recalcitrant experience. To follow Quine's image, the most synthetic are considered nearest experience - on the periphery of our system, whereas the most analytic sentences are nearer the centre of the system. In a sense, those sentences at the centre define the system, because one could say that when one has abandoned those sentences one has abandoned the system. The sentences at the periphery, though, the so-called 'observation sentences' (-they wear their meaning on their sleeves-) are conditioned to experience in a referential sense. These are used as the links to experience- the surface structure- which enable the community of scientists to compare and evaluate competing theories (deep structures).

The D-thesis applies at every level of science. At the level of 'normal' science the decision is to hold the background theory as true and to let the hypothesis go as 'falsified' in the face of disconfirmation. At the level of 'extraordinary' science, on the other hand, the background is tampered with in order to preserve a prized hypothesis. It is important to note that when one claims that the truth of a presumably 'falsified' hypothesis H is preserved by making the appropriate adjustments, one is not using the words 'true' and 'false' in the same sense that they are used when one considers the unit of significance to be the statement. One significant feature of the D-theoretic position is to point out that the unit of empirical significance is the system of statements (-a much broader unit). From the D-theoretic point of view, statements can never be sufficiently isolated to be true and false by themselves and hence are considered instead to be 'contextually true' and 'contextually false'. Under such a construal the 'truth' of an individual hypothesis is preserved by altering its context in some way.

One can interpret the D-theoretic position as a return to a holistic science. Science advanced when it discarded Aristotelian holistic techniques and adopted the piecemeal mechanical approach that was eventually to be used with considerable success in the mechanics of Newton. Since science has been so enormously successful using such techniques, why advocate a return to an holistic approach?

The answer is basically economic. A **laissez-faire** attitude to the pursuit of knowledge is no longer feasible. So long as scientists conducted simple and inexpensive experiments in isolation from the rest of the world, no apparent harm was done to the earth or its inhabitants and mankind reaped vast technological rewards. Now, however, certain types of experiments -such as, those needing the construction of elaborate nuclear accelerators, the production of complex space craft, or the completion of large-scale computer installations- are costly to society in terms of dollar costs. Other experiments and projects - such as those involving the use of atomic energy, the creation of massive dams, the production of certain metals and chemicals- are costly in terms of ecological damage. Societies have limited resources and man must continue to live on this earth long after this generation. We cannot, therefore, be too casual in the way that we dispose of our natural resources. Priorities need to be set which determine the effective utilization of societies' resources. A global picture of science depicting the subtle inter-relationships and inter-connections among the parts of science would be helpful in the task of priority-setting. A view of science that displays the role that our interests and purposes play in the formation of our knowledge would also be useful. The creation of such a global view of science would be a large task. The D-thesis is valuable because it suggests an epistemological starting-point for such a project.

APPENDIX ONE

Duhem's Thesis

D-thesis is the name that Adolf Grünbaum gave to Quine's thesis that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system,"[522] Grünbaum called it the D-thesis after Pierre Duhem[523], a French physicist, who expressed a similar thesis in his book ***The Aim and Structure of Physical Theory***. Quine's thesis, however, goes much further than Duhem was prepared to go.

Duhem's thesis is found in the sixth chapter of his book where he discusses the relationship between physical theory and experiment. He gives an indication of the thesis in the title of the second section of that chapter, "An Experiment in Physics Can Never Condemn an Isolated Hypothesis but Only a Whole Theoretical Group."[524]

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522 W.V.O. Quine, 'Two Dogmas of Empiricism', ***From a Logical Point of View*** (New York, 1963). p. 43

523 Grünbaum gave the name D-thesis to Quine's thesis in the fourth chapter of his book ***Philosophical Problems of Space and Time*** (New York, 1963) p. 108. Presumably, he had noticed the reference to Duhem in Quine's 'Two Dogmas of Empiricism'. In that article Quine has a footnote in which he tells us that the thesis that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" ("Two Dogmas", p. 11.) has been "well argued by Duhem." ("Two Dogmas", p. 41.) As we shall see Quine's thesis is very similar to Duhem's in many of its aspects.

524 Pierre Duhem, ***The Aim and Structure of Physical Theory*** (Princeton, 1954) transl. P.P. Wiener p. 183.

In that section, he makes it clear that the evaluation of an experimental result is not a simple matter.

> A physicist decided to demonstrate the inaccuracy of a proposition; in order to deduce from this proposition the prediction of a phenomenon and institute the experiment which is to show whether this phenomenon is or is not produced, in order to interpret the results of this experiment and establish that the predicted phenomenon is not produced, he does not confine himself to making use of the proposition in question; he makes use also of a whole group of theories accepted by him as beyond dispute. The prediction of the phenomenon, whose nonproduction is to cut off debate, does not derive from the proposition challenged if taken by itself, but from the proposition at issue joined to that whole group of theories; if the predicted phenomenon is not produced, not only is the proposition questioned at fault, but so is the whole theoretical scaffolding used by the physicist. The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error, but where this error lies is just what it does not tell us.[525]

Duhem's thesis is only one half of what we are calling Quine's D-thesis. Laurens Laudan[526] has pointed out that Quine's D-thesis is much stronger than Duhem's claim. Laudan's point is that Duhem wished to show chiefly that falsification is as inconclusive as verification and later that crucial experiments were impossible. Quine's D-thesis goes much further than this and claims that a revision can be found to save any falsified hypothesis. Any hypothesis can be saved "come what may". Laudan tells us that according to Duhem an hypothesis is not falsified unless someone has proven that it cannot be saved, - that is, unless someone can prove that no revision can be found. Thus the onus

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This book was originally published in French under the title ***La Théorie Physique: Son Objet, Sa Structure*** 2nd ed. (Paris, 1911).

525 ***Ibid***., p. 185

526 Laurens Laudan, "Discussion: Grünbaum on 'The Duhemian Argument'", ***Philosophy of Science*** vol. 32 (1965) pp. 295-299.

of proof falls on those claiming the separate falsifiability of the hypothesis. With Quine's D thesis, however, the onus falls on the D-theorist to supply the appropriate revision.

Philip Quinn, a student of Grünbaum, has distinguished two sub-theses of Quine's D-thesis. The first, which is Duhem's thesis that no hypothesis is separately falsifiable, he calls D_1. D_1 states that "no hypothesis H which is constituent of any scientist's theory can ever be sufficiently isolated from some set of auxiliary assumptions or other so as to be separately falsifiable by observations."[527] The second sub-thesis, called D_2, is the stronger thesis that for every falsified hypothesis some revision can be found to save the hypothesis in question. D_2 states that "for every hypothesis H, auxiliary assumption A, and observational statements O and O' such that $(H \& A) \rightarrow O$, and O' and $\sim (O \& O')$, there is an A' such that H & A' can be held true and H & A' explains O'."[528]

Duhem was aiming his attack at the popular misconception "that each one of the hypotheses employed in physics can be taken in isolation, checked by experiment, and then, when many varied tests have established its validity, given a definitive place in the system of physics."[529] Duhem has an organic view of physical science. One can see a considerable similarity between his view and the view expressed by Quine near the end of his "Two Dogmas..." paper. Duhem says:

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527 Philip Quinn, "The Status of the D-thesis", ***Philosophy of Science*** vol. 36 no. 4. (Dec., 1969) p. 398.

528 ***Ibid***.

529 Duhem, ***op. cit***., p. 187.

> Physical science is a system that must be taken as a whole; it is an organism in which one part cannot be made to function except when the parts that are most remote from it are called into play, some more so than others, but all to some degree. If something goes wrong, if some discomfort is felt in the functioning of the organism, the physicist will have to ferret out through its effect on the entire system which organ needs to be remedied or modified without the possibility of isolating this organ and examining it apart.[530]

Duhem regards physical science as an organic whole. The parts of science are interrelated in much the same way that the parts of the human body are related. A malfunction in one part of the organism may affect the working of other parts and may even prevent the proper functioning of the whole body.

Duhem also used his thesis to attack the belief in crucial experiments. Some scientists believed that on the basis of experiment one could definitively decide against one of two competing hypotheses. Duhem presented the method as follows:

> Seek experimental conditions such that one of the hypotheses forecasts the production of one phenomenon and the other the production of quite a different effect; bring these conditions into existence and observe what happens; depending on whether you observe the first or second of the predicted phenomena, you will condemn the second or the first hypothesis; the hypothesis not condemned will be henceforth indisputable, debate will be cut off, and a new truth will be acquired by science.[531]

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530 ***Ibid***., p. 188.

531 ***Ibid***.

Duhem objects, though, that such an experiment does not decide between two hypotheses, but "it decides rather between two sets of theories each of which has to be taken as a whole, i.e., between two entire systems..."[532]

Another problem with crucial experiments is that they cannot decisively find in favour of an hypothesis because there are no strict dilemmas in physical science as there is in geometry. Geometry, Duhem says, can decide between two contradictory theorems by using the familiar **reductio ad absurdum** technique.

> Unlike the reduction to absurdity employed by geometers, experimental contradiction does not have the power to transform a physical hypothesis into an indisputable truth; in order to confer this power on it, it would be necessary to enumerate completely the various hypotheses which may cover a determinate group of phenomena; but the physicist is never sure he has exhausted all the imaginable assumptions.[533]

That is, although an experiment might favour an hypothesis H_1 over an hypothesis H_2, one never knows whether or not there is an hypothesis H_3 which is better than H_1 or even whether or not there might be some hypothesis H_4 which in conjunction with H_2 would give a better prediction than H_1. The possibilities for alternative hypotheses is enormous and so no so-called crucial experiment is final. It is crucial only in a very limited way.

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532 ***Ibid***., p. 189.

533 ***Ibid***., p. 190.

In Quine's field theory of knowledge, there seems to be a hierarchy of statements proposed for any particular field. In determining what revisions are to be made in the light of recalcitrant experience, he considers certain statements about physical objects to be close to the periphery of his field, that is, close to experience. He considers these statements "to have a sharper empirical reference than highly theoretical statements of physics or logic or ontology."[534] In other words, he would be inclined to alter these more empirical or less theoretical statements first in the face of recalcitrant experience since this would disturb "the total system as little as possible."[535] (Presumably, these statements would not have as many logical connections as the more theoretical statements). The highly theoretical statements or those whose alteration would disturb the total system considerably, Quine considers to be centrally located in the network and are altered last.

On Quine's view, there is no distinction in kind of statements, rather statements differ in degree according to how they link up with other statements in the total system. Those statements which are logically linked to a large number of other statements, the more theoretical, are given up only after statements with fewer logical links, the more empirical. We, thus, have a hierarchy of statements. The position of a particular statement in the hierarchy is somehow determined by the number of logical links with other statements. Those with the most logical links would rank as statements to be given up last in the face of recalcitrant experience.

It is interesting to note that Duhem has a similar hierarchy.

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[534] Quine, ***op. cit***., p. 44.

[535] ***Ibid***.

> Theory is in principle grounded on postulates, that is to say, on propositions that it is at leisure to state as it pleases, provided that no contradiction exists among the terms of the same postulate or between two distinct postulates. But once these postulates are set down it is bound to guard them with jealous rigor. For instance, if it has placed at the base of its system the principle of the conservation of energy, it must forbid any assertion in disagreement with this principle.[536]

According to Duhem, these fundamental postulates cannot be directly tested through a crucial experiment, rather the whole theory when completely developed faces the judgment of experience. Duhem agrees with a principle put forward by his contemporaries G. Milhaud, H. Poincaré, and Edouard Le Roy that:

> Certain fundamental hypotheses of physical theory cannot be contradicted by any experiment, because they constitute in reality ***definitions***, and because certain expressions in the physicist's usage take their meaning through them.[537]

Duhem's view is that when there is a disagreement between the symbolic representation of the theory and the experimental result, this "proves that some part of this symbol is to be rejected," but the experiment does not tell us which part is to be changed. Like Quine, he believes that there are certain statements that are given up last. He writes:

> Now among the theoretical elements entering into the composition of this symbol there is always a certain number which the physicists of a certain epoch agree in accepting without test and which they regard as beyond dispute. Hence, the physicist who wishes to modify this

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[536] Duhem, ***op. cit***., p. 206.

[537] ***Ibid***., p. 209.

> symbol will surely bring his modification to bear on elements other than those just mentioned.[538]

Again, as in Quine's theory, Duhem believed that even these most fundamental statements are not immune from revision, for he cautions:

> Indeed, we must really guard ourselves against believing forever warranted those hypotheses which have become universally adopted conventions, and whose certainty seems to break through experimental contradiction by throwing the latter back on more doubtful assumptions. The history of physics shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms, and to rebuild its physical theories on new hypotheses.[539]

Duhem's theory is very similar to Quine's field theory of knowledge. He has a hierarchy of statements and also seems to hold that there is no real distinction in kind among statements since even the most fundamental statements are not immune from revision.

It is evident that Duhem was fully aware of the fundamental importance of his doctrine since he spoke out against those who taught an inaccurate view of the relationship between experiment and theory. He wrote:

> The teaching of physics by the purely inductive method such as Newton defined it is a chimera. Whoever claims to grasp this mirage is deluding himself and deluding his pupils. He is giving them, as facts see, facts merely foreseen; as precise observations, rough reports;

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[538] ***Ibid***., p. 211.

[539] ***Ibid***., p. 212.

> as performable procedures, merely ideal experiments; as experimental laws, propositions whose terms cannot be taken as real without contradiction. The physics he expounds is false and falsified.[540]

Here Duhem is referring to those teachers who believe and teach that every hypothesis of physics is separately testable and verifiable and that as each is sufficiently verified it is accepted and added as a building block to the whole structure of physics. Duhem advocates that the correct conception of scientific method be taught.

> If the interpretation of the slightest experiment in physics presupposes the use of a whole set of theories and if the very description of this experiment requires a great many abstract symbolic expressions whose meaning and correspondence with the facts are indicated only by theories, it will indeed be necessary for the physicist to decide to develop a long chain of hypotheses and deductions before trying the slightest comparison between the theoretical structure and the concrete reality; also, in describing experiments verifying theories already developed, he will very often have to anticipate theories to come.[541]

In this way, the student should learn the proper relationship between experiment and theory and by doing so he will be a better scientist and we shall have a better science. Duhem recommends that:

> Instruction ought to get the student to grasp this primary truth: Experimental verifications are not the base of theory but its crown. Physics does not make progress in the way geometry does: the latter grows by the continual contribution of a new theorem demonstrated once and for all and added to theorems already demonstrated; the former is a symbolic painting in which continual retouching gives greater comprehensiveness and unity, and the ***whole*** of which gives a picture resembling more and more the ***whole*** of the experimental facts, whereas

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[540] ***Ibid***., pp. 203-4.

[541] ***Ibid***., p. 204.

each detail of this picture cut off and isolated from the whole loses all meaning and no longer represent any thing.[542]

[542] ***Ibid***., p. 205.

APPENDIX TWO

Quine's Development of Stimulus Meaning

Stimulus meaning is by no means to be taken as a full account of meaning. It only comes close to being a full account of meaning for only the most empirical sentences.

The sentence 'Bachelor' provides a counter-example to the notion that stimulus meaning is all there is to meaning. Quine writes: "The stimulus meaning of 'Bachelor' cannot be treated as its "meaning" by any stretch of the modulus."[543] The reason for this is the amount of collateral information that is required to determine the truth or falsity of a sentence like 'Bachelor' (presumably said while indicating someone). An assent or dissent from such a sentence relies a great deal on previous information and very little on the prompting stimulation. One has to know quite a bit about the individual indicated. The evaluation of a sentence like 'Red' or 'This is red' usually does not involve a great deal of collateral information.

One can begin to order sentences according to the effects of collateral information. For example, observation sentences are defined as: "Occasion sentences whose stimulus meanings vary none under the influence of collateral information."[544] As Quine puts it: "These are the occasion

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[543] W.V.O. Quine, ***Word and Object*** (Cambridge, 1960) p. 42.

[544] Quine, ***Word... op. cit***., p. 42.

sentences that wear their meanings on their sleeves."[545] In the case of observation sentences "their stimulus meanings may without fear of contradiction be said to do full justice to their meanings."[546] Sentences are thus ranked according to degrees of observationality. Quine says that: "What we have is a gradation of observationality from one extreme, at 'Red' or above to the other extreme at 'Bachelor' or below."[547] However, even the stimulus meaning of 'Red' can "be made to fluctuate a little from occasion to occasion by collateral information on lighting conditions."[548]

In order to 'get at' the meaning of Quine's remarks we have to make clear what is meant by "stimulus meaning', 'collateral information', 'occasion sentence', 'modulus' and the like.

Quine defines 'stimulus meaning' as the ordered pair of affirmative stimulus meaning and negative stimulus meaning.

> ... a stimulation σ belongs to the affirmable stimulus meaning of a sentence S for a given speaker if and only if there is a stimulation σ' such that if the speaker were given σ' then were asked S. then were given σ, and then were asked S again, he would dissent the first time and assent the second. We may define the negative stimulus meaning similarly with 'assent' and 'dissent' interchanged, and then define the stimulus meaning as the ordered pair of the two.[549]

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545 ***Ibid***.

546 ***Ibid***.

547 ***Ibid***.

548 ***Ibid***.

549 ***Ibid***., pp. 32-3. This definition holds for most sentences, although in case of some extreme sentences usually labelled as analytic sentences assent is given regardless of the stimulation. This will be discussed in more detail later.

Presumably, then, negative stimulus meaning would be defined as follows: a stimulation σ belongs to the negative stimulus meaning of a sentence S for a given speaker if and only if there is a stimulation σ' such that if the speaker were given σ' , then were asked S, then were given σ and then were asked S again, he would assent the first time and dissent the second time.

Quine is here defining stimulus meaning for sentences that one can assent to or dissent from. Certainly not all sentences are of this form. Also his analysis presupposes that one can recognize when an individual assents to or dissents from the sentence on the occasion of the stimulus. The stimulus, of course, could involve a variety of surface irritations, be they visual, aural, tactile, olfactory, or gustatorial. Obviously, then, stimulus meaning introduces the empirical element into Quine's theory for it involves a direct reaction by an individual to an empirical stimulus.

Important to the concept of stimulus meaning is the notion of the **modulus** of stimulation. The modulus of stimulation is the "bound we set to the length of stimulations counted as current."[550] In other words, the modulus of stimulation is "a working standard of what to count as specious present."[551] One problem in determining the stimulus meaning of a sentence has to do with eliminating as well as possible the influence of collateral information. Quine is concerned to capture "language as the complex of present dispositions to verbal behavior, in which speakers of the same

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[550] ***Ibid***., p. 28.

[551] ***Ibid***.

language have perforce come to resemble one another."[552] He makes it quite clear that he is not concerned with the process of acquiring a language since this process varies from individual to individual according to the particular experiences of the individual. Thus in spelling out the notion of stimulus meaning one has to be careful to reduce the modulus of stimulation in order to eliminate effects due to past stimulation or due to acquisition of collateral information. This becomes clearer if one considers a sentence such as "That man shoots well."[553] Quine writes:

> The sentence 'That man shoots well', said while pointing to an unarmed man, has as present stimulation the glimpse of the marksman's familiar face. The contributory past stimulation includes past observations of the man's shooting, as well as remote episodes that trained the speaker in the use of words.[554]

To get at the base stimulus meaning of a sentence, one has to select an appropriately small modulus of stimulation. The selection of such a modulus is meet with a great variety of problems which I will ignore for the present. Let it suffice to say that for a given sentence if it is to be understood by an individual such under standing will involve some past stimulation and even some collateral information. In each case, though, the amount of each will vary. Obviously, if an individual is going to understand a sentence he will at least need to have had some exposure to the sounds uttered. All that is part of learning a language. So naturally there will be no such thing as

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[552] ***Ibid***., p. 27.

[553] ***Ibid***.

[554] ***Ibid***.

pure stimulus meaning. If an individual is meaningfully going to assent to or dissent from a particular sentence in a particular situation, then minimally he must understand the sentence.

Granting that it is impossible to have any such thing as pure stimulus meaning - or meaning without appeal to previous stimulation or collateral information - we must recognize that with the concept of stimulus meaning Quine has the crude beginnings for a theory of empirical meaning.

Quine, for example, defines "... the affirmative stimulus meaning of a sentence such as 'Gavagai', for a given speaker, as the class of all stimulations (hence evolving ocular irradiation patterns between properly timed blindfoldings) that would prompt his assent."[555] (In this case 'Gavagai' is taken to be a sentence from an as yet untranslated native language. The idea is to discover the stimuli that would prompt assent to such an utterance.) Put this way, then, stimulus meaning is a dispositional affair.[556] It indicates a disposition on the part of an individual to assent to or dissent from certain sentences under specific circumstances. Quine writes: "The stimulus meaning of a sentence for a subject sums up his disposition to assent to or dissent from the sentence in response

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555 ***Ibid***. p. 32. Quine uses the word 'Gavagai' in his example of radical translation. Radical translation is the translation of a previously unknown language. As Quine puts it:

> "The utterances first and most surely translated in such a case are ones keyed to present events that are conspicuous to the linguist and his informant. A rabbit scurries by, the native says 'Gavagai', and the linguist notes down the sentence 'Rabbit' (or 'Lo, a rabbit') as tentative translation, subject to testing in further cases. The linguist will at first refrain from putting words into his informant's mouth, if only for lack of words to put. When he can, though, the linguist has to supply native sentences for his informant's approval, despite the risk of slanting the data by suggestion. Otherwise he can do little with native terms that have references in common." (***Word and Object***, p. 29.)

556 See pp. 33-4 in ***Word and Object***.

to present stimulation."[557] This is in keeping with his earlier pronouncement on meaning; to determine the significance of a linguistic utterance by analyzing "it in terms directly of what people do in the presence of the linguistic utterance in question and other utterances similar to it."[558] Stimulus meaning indicates the response of a particular individual at a particular time to a particular sentence utterance. This permits the individual to respond differently at another time to another utterance of the same sentence. Such behavior would indicate a change in the stimulus meaning of a particular sentence for that individual.

Not only may stimulus meaning vary over particular dates, but it may vary according to the length of the modulus of stimulation or as Quine calls it "the maximum duration recognized for stimulations."[559] He writes: "For, by increasing the modulus we supplement the stimulus meaning with some stimulations that were too long to count before."[560]

Quine summarizes as follows: "Fully ticketed, therefore, a stimulus meaning is the stimulus meaning **modulo n** seconds of sentence S for speaker **a** at time **t**."[561]

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557 ***Ibid***., p. 31.

558 Quine, "On What There Is", ***From... op. cit***. p. 11.

559 Quine, ***Word... op. cit***., p. 33.

560 ***Ibid***.

561 ***Ibid***.

The reason why Quine includes negative as well as affirmative stimulus meanings in the concept of stimulus meaning is because the one does not determine the other. There may be many stimulations that are irrelevant[562] and belong to neither affirmative or negative stimulus meaning.[563]

Quine views stimulus meaning as "a device, as far as it goes, for exploring the fabric of interlocking sentences, a sentence at a time."[564] The notion of stimulus meaning, thus, is Quine's attempt to get at the "net empirical import of various single sentences without regard to the containing theory, even though without loss of what the sentence owes to that containing theory."[565] This, then offers the way around the relativity involved in talking about the meaning of individual sentences in the situation where the unit of empirical significance is taken to be the whole theory. Quine does not claim that the notion of stimulus meaning gets us cleanly out of that predicament, but he claims only that "the notion of stimulus meaning partially resolves the predicament."[566] As we mentioned above, if a speaker or individual is to understand the sentence being queried he must have had previous experience such as an appeal to other stimulation and to collateral information.

You will recall Frankfurt's complaint about the development of Quine's suggestion that the unit of empirical significance is the whole of science. Quine has indicated that the device of stimulus

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[562] See p. 36. in ***Word and Object***.

[563] See p. 33. in ***Word and Object***.

[564] ***Ibid***., p. 35.

[565] ***Ibid***., pp. 31-5.

[566] ***Ibid***. p. 34.

meaning helps us to unravel the fabric of the language of science. It does this by helping us to distinguish between the degrees of observationality of certain sentences. Every sentence of the language has an empirical component, but some sentences are more empirical then others. There are degrees of observationality.

Quine begins to classify sentences according to their observationality. He distinguishes occasion sentences from standing sentences. "**Occasion** sentences, as against standing sentences, are sentences such as 'Gavagai', 'Red', 'It hurts', 'his face is dirty', which command assent or dissent only if queried after an appropriate prompting stimulation."[567]

What Quine seems to be getting at is the immediacy of the judgment in the case of the occasion sentence. His examples of standing sentences are: "There is ether drift", "The crocuses are out" and "The Times has come."[568] These sentences can also be prompted. He points out that "stimulation implemented by an interferometer once prompted Michelson and Morley to dissent from the standing sentence 'There is ether drift', and a speaker's assent can be prompted yearly to 'The crocuses are out', daily to 'The Times has come.'"[569] The difference between occasion sentences and standing sentences is one of degree. A standing sentence is one to which the individual "may repeat his old assent and dissent unprompted by current stimulation when we ask him again on later

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[567] ***Ibid*** . pp. 35-6.

[568] ***Ibid***., p. 36.

[569] ***Ibid***.

occasions."[570] Once the Times has come during the day and that it has come has been noted, then one may assent to the sentence, 'The Times has come' without another delivery of the Times that day. An occasion sentence, on the other hand, "commands assent or dissent only as prompted all over again by current stimulation."[571] We may assent to the sentence 'His face is dirty' upon the visual stimulation of seeing his face and finding that it is dirty, but we cannot assent to that same sentence an hour later (particularly if we know he washed up in the meantime) unless we again see his face. One can easily see from this last example that the distinction is one of degree, for whether or not one needs reprompting in this case is a question of time. One may assent to the sentence 'His face is dirty' five seconds after the first assenting without reprompting if we know he has not had time to clean it (or if we know that some one has not turned a hose on him.) One might say that this was still within the modulus of stimulation. As Quine neatly puts it:

> Standing sentences grade off toward occasion sentences as the interval between possible repromptings diminishes; and the occasion sentence is the extreme case where that interval is less than the modulus.[572]

So the distinction is relative to the modulus of stimulation.

As we have already noted, stimulus meanings are not pure but vary for an individual depending upon his prior history and various amounts of collateral information. "Some stimulus

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[570] ***Ibid***.

[571] ***Ibid***.

[572] ***Ibid***.

meanings are less susceptible than others to the influences of intrusive information."[573] For example, the sentence 'Bachelor' involves a great deal more of what Quine calls intrusive information than a sentence like 'Red'.[574] One has to know a lot more about the individual to know that he is a bachelor than what shows on the surface. As Quine puts it: "the trouble with 'Bachelor' is that its meaning transcends the looks of the prompting faces and concerns matters that can be known only through other channels."[575]

You will recall that Quine defined an observation sentence as an occasion sentence[576] "whose stimulus meanings vary none under the influence of collateral information."[577] In their case, stimulus meaning captures their meaning. These would be counted as the most empirical of the sentences of our language. These are the sentences that tie our language to the world by having direct correspondence to stimulation. As Quine puts it:

> To philosophers 'observation sentence' suggests the datum sentences of science. On this score our version is not amiss; for the observation sentences as we have identified them are just the occasion sentences on which there is pretty sure to be firm agreement on the part of

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[573] ***Ibid.***, p. 10.

[574] These sentences are uttered, presumably, while pointing or indicating in some manner some person or object.

[575] ***Ibid.***, p. 42.

[576] Quine does not define observationality in terms of standing sentences "since the stimulus meaning of a standing sentence can show fair constancy from speaker to speaker for the wrong reason: mere sparseness of member stimulations." (***Ibid.***, p. 43.)

[577] ***Ibid.***, p. 12.

> well-placed observers. Thus they are just the sentences on which a scientist will tend to fall back when pressed by doubting colleagues.[578]

To put it another way, observation sentences are just those sentences on which there is a great deal of intersubjective agreement. "What makes an occasion sentence low on observationality is, by definition, wide intersubjective variability of stimulus meaning.[579]

Thus, we can see how the notion of stimulus meaning provides the empirical basis for Quine's field theory.[580] The empirical footings are provided by the observation sentences or the sentences

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578 ***Ibid***., p. 44. Quine notes that his "version of observation sentences departs from a philosophical tradition in allowing the sentences to be about ordinary things instead of requiring them to report sense data ..." (***Ibid***., p. 44.) In the book that Quine has co-authored with Ullian, ***The Web of Belief*** (New York, 1970) we read:

> "The special virtue of observation sentences is that we can in principle learn them by ostension as wholes, keyed as wholes to the appropriate observable occasions, before ever learning to link the component words to enduring bodies. "The cat is on the mat' can be learned ostensively as a unitary string of syllables in association with a certain range of possible scenes. All of us necessarily learned some observation sentences thus. Then, as we gradually caught on to the theory of enduring bodies, we came to attribute corporeal reference to component words. Learning by ostension, as a trained animal might, to associate whole observation sentences with appropriate patterns of stimulation is a first indispensible step toward learning physical theory." (***The Web of Belief***, p. 15)

Later on, they write:

> "Probably none of us in fact learned "The cat is on the mat" outright by ostension, but we could have. A likelier example is "(This is a)ball," or "Yellow," or "Mamma." An important trait of language is that people learn it by different routes and no record of the route is preserved in the words learned. What makes a sentence an observation sentence is not that it **was** learned ostensively but that it is of a sort that **could** have been." (p. 16.)

579 ***Ibid***., p. 45.

580 Readers who wish to delve much more deeply into the notion of stimulus meaning are directed to the second chapter of ***Word and Object***.

whose meanings are wholly captured by stimulus meaning. These sentences are connected in a complex way to the other sentences of the language. The next task in unravelling the fabric of language is to try to capture just what these connections are. Just what are the relationships between the observation sentences and the other sentences of the language?

Quine views language "as a socially inculcated set of dispositions" and as such it "is substantially uniform over the community, but it is uniform in different ways for different sentences."[581] The structure of language or the interrelationships among the sentences of a language are not easily ferreted out. With sentences other than observation sentences there will be a considerable amount of variation in their stimulus meanings. One individual, a physicist, may assent to a sentence where most other individuals usually dissent. This may be because of some basic information that he has that the others do not have. Quine writes:

> If a sentence is one that (like 'Red' and 'Rabbit') is inculcated mostly by something like direct ostension, the uniformity will lie at the surface and there will be little variation in stimulus meaning; the sentence will be highly observational. If it is one that (like 'Bachelor') is inculcated through connections with other sentences, linking up thus indirectly with past stimulations of other sorts than those that serve directly to prompt present assent to the sentence, then its stimulus meaning will vary with the speaker's past, and the sentence will count as very unobservational.[582]

It is these vague connections with past stimulations peculiar to a person's history that create the difficulty in capturing the interrelationships between sentences. As Quine puts it:

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581 ***Ibid***., p. 45.

582 ***Ibid***.

> The stimulus meaning of a very unobservational occasion sentence for a speaker is a product of two factors, a fairly standard set of sentence to sentence connections and a random personal history; hence the largely random character of the stimulus meaning from speaker to speaker.[583]

One, thus, has to devise a way of getting around these random aspects in order to get at the sentence to sentence connections. This is where Quine employs his notion of stimulus synonymy. By means of stimulus synonymy one can get at the relationship between certain occasion sentences such as 'Bachelor' and 'Unmarried man' or 'Buffalo nickel', and 'Indian nickel'.[584] Naturally, there are problems with this notion.

As we have already noted, stimulus meaning does not even come close to capturing the meaning of a non-observational sentence such as 'Bachelor'. However, according to Quine, even though the synonymy of 'Bachelor' and 'Unmarried man' cannot be predicted on the identity of stimulus meaning, the stimulus meanings of 'Bachelor'[585] and 'Unmarried man' are "identical for any one speaker."[586] That is, an individual would be prompted to assent to or dissent from 'Bachelor' under the same stimulations as he would assent to or dissent from 'Unmarried man'. Thus Quine concludes that sameness of stimulus meaning or what he calls **stimulus synonymy** "is as good a standard of synonymy for non-observational occasion sentences as for observation Here he

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583 ***Ibid***.

584 All said while indicating some particular person or thing.

585 Here, we are following Quine in leaving aside complications due to meanings such as bachelor of arts.

586 ***Ibid***., p. 46.

sentences as long as we stick to one speaker."[587] Here he is not claiming stimulus synonymy as a standard for the sameness of meaning for clearly 'Bachelor' and 'Unmarried man' differ in meaning.[588] As Quine puts it:

> For each speaker 'Bachelor' and 'Unmarried man' are stimulus-synonymous without having the same meaning in any acceptably defined sense of 'meaning' (for stimulus meaning is, in the case of 'Bachelor', nothing of the kind). Very well, here is a case where we may welcome the synonymy and let the meaning go.[589]

Stimulus synonymy, of course, also involves the problem of collateral information. Quine points to the sentences 'Indian nickel' and 'Buffalo nickel'. For someone ignorant of American coins, these two sentences will not have the same stimulus meaning. However, if it has been pointed out that on one side of the nickel there is an Indian and on the other side there is a buffalo, then an individual would tend to assent to (or dissent from) 'Indian nickel' under the same promptings as he would assent to (or dissent from) 'Buffalo nickel'. One thing that this example shows is that "two terms can in fact be coextensive,

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587 ***Ibid***.

588 They agree in extension on a very broad view of extension (here, of course, we are alluding to the problem of whether rabbits, rabbit stages, or undetached rabbit parts, etc. are being designate) which is what stimulus meaning shows, but they certainly do not agree in intension.

589 ***Ibid***. Quine here briefly discusses discrepancies in stimulus meanings due to interference from other languages, shock, second intention use of words, sentence length, etc.

or true of the same things, without being intrasubjectively stimulus-synonymous as occasion sentences."[590] This was the case of individuals who did not have the collateral information about American coins.

Quine stresses in ***Word and Object*** that "words are learned only by abstraction from their roles in learned sentences."[591] So far in his consideration of stimulus meaning and stimulus synonymy, he has been dealing with one word sentences. Since he has found that for certain sentences, observation sentences, the concept of stimulus meaning can serve as the meaning of those sentences, he wonders whether this concept would also provide a way of getting at the meaning of the terms used in these sentences. For example, the terms 'red' and 'rabbit' in the sentences 'Red' and 'Rabbit'. He concludes that it cannot be used in this way because in the case of his two sentences 'Gavagai' and 'Rabbit' (where one is the translation of the other because of similar stimulus meaning) one cannot be sure that the terms 'rabbit' and 'gavagai' are coextensive. The term 'gavagai' may not refer to rabbits at all but to rabbit stages, or undetached rabbit parts, etc. As Quine pointed out in his paper "Identity, Ostension, and Hypostasis" (1950) in order to distinguish the referent we need also use identity as well as ostension. That is, pointing must be "accompanied by questions of identity and diversity: 'Is this the same gavagai as that?', 'Do we have here one gavagai or two?'"[592]

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590 ***Ibid***., p. 51.

591 ***Ibid***.

592 ***Ibid***., p. 53.

Stimulus meanings, therefore, cannot get us to the meaning of terms. Terms are cultural to the extent that they are peculiar to particular conceptual schemes. In spite of this relativity, stimulus meanings may still match up. "Occasion sentences and stimulus meanings are general coin; terms and reference are local to our conceptual scheme."[593] Thus, stimulus meanings and stimulus synonymy cannot provide for the meanings of terms. This is chiefly, though, a problem in the translation of terms. Within the same language, however, Quine has devised a way to get synonymy of terms from the stimulus synonymy of the corresponding occasion sentences.

> To get synonymy of terms from synonymy of the corresponding occasion sentences we need only add a condition that will screen out such pairs as 'bachelor' and 'part of bachelor'; and this we can do by requiring that the subject be prepared to assent to the standing sentence 'All Fs are Gs and vice versa', thinking of 'F' and 'G' as the terms in question. The definition becomes this: 'F' and 'G' are stimulus-synonymous as terms for a speaker at **t** if and only if as occasion sentences they have the same stimulus meaning for him at t and he would assent to 'All Fs are Gs and vice versa' if asked at **t**.[594]

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[593] ***Ibid***. All this overlaps with Quine's discussion of radical translation which we have been leaving outside of this discussion in order to keep our discussion as uncomplicated as possible. But we should remark that this indicates the essential problem with radical translation. As Quine writes here:

> "We cannot even say what native locutions to count as analogues of terms as we know them, much less equate them with ours term for term, except as we have also decided what native devices to view as doing in their devious ways the work of our own various auxiliaries to objective reference: our articles and pronouns, our singular and plural, our copula, our identity predicate. The whole apparatus is interdependent, and the very notion of term is as provincial to our culture as are those associated devices. The native may achieve the same net effects through linguistic structures so different that any eventual construing of our devices in the native language and vice versa can prove unnatural and largely arbitrary." (***Ibid***., p. 53.)

[594] ***Ibid***., p. 54. Here, of course, he is relying on a form of identity (which can be used if we already understand the language, that is, if the significance of 'all' and 'are' are settled in advance) in addition to stimulus meaning in order to get to the meaning of the terms of our language.

One way that Quine suggests for removing the effects of collateral information on stimulus synonymy is to socialize the concept. That is, "we can count just those terms as socially stimulus-synonymous that come out stimulus-synonymous for each individual speaker almost without exception."[595] Under the concept of social stimulus synonymous, 'bachelor' and 'unmarried man' are stimulus synonymous where 'Indian nickel' and 'buffalo nickel' are not, because there are some individuals who are not acquainted with American coins.

Quine notes that the difference between the situation with 'bachelor' and 'unmarried man' and the situation between 'Indian nickel' and 'buffalo nickel' probably indicates a difference in how one comes to learn these terms. "We learn 'bachelor' by learning appropriate associations of words with words, and 'Indian nickel' by learning directly to associate the term with sample objects."[596] Quine claims that 'bachelor' is semantically anchored[597] to 'unmarried man'. He writes: "...sever its tie with 'unmarried man' and you leave it no evident social determination hence no utility in communication."[598] Besides learning words by (a) learning appropriate associations of words with words or (b) learning directly to associate the term with sample objects, there is yet a third way. This is the way that we learn many terms of theoretical science. Quine claims that these terms are like 'bachelor' "in having no socially constant stimulus meanings to govern their use."[599] He remarks,

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[595] ***Ibid***., p. 55.

[596] ***Ibid***., p. 56.

[597] ***Ibid***.

[598] ***Ibid***.

[599] ***Ibid***.

however, that they differ "in having a more complex network of verbal connections, so that no one tie seems crucial to communication."[600] These terms tie into language in more than just one way so their synonymies are not as easily captured as in the case of 'bachelor'. Their connections to other parts of language are not so apparent. They reflect the intricate complexity of a fully developed language.[601]

After having dealt somewhat with the role of terms, Quine now moves into the examination of more complex parts of language. In particular, he discusses the role of logical connectives.

In his discussion, Quine deals with truth functions such as negation, logical conjunctions, and alternation. The discussion is conducted in terms of discovering out these logical connectives in a previously untranslated language. He easily comes up with semantic criteria in terms of assent and dissent by which a translator could determine whether a particular construction in the language being translated expressed a particular truth function. He gives these criteria as follows:

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[600] ***Ibid***., p. 57.

[601] Quine writes:

> "Thus it is that in theoretical science, unless as recast by semantics enthusiasts, distinctions between synonymies and "factual" equivalences are seldom sensed or claimed. Even the identity historically introduced into mechanics by defining 'momentum' as 'mass times velocity' takes its place in the network of connections on a par with the rest; if a physicist subsequently so revises mechanics that momentum fails to be proportional to velocity, the change will probably be seen as a change of theory and not peculiarly of meaning. Synonymy intuitions do not emerge here, just because the terms are linked to the rest of language in more ways than words like 'bachelor' are." (***Ibid***., p. 57.)

> The semantic criterion of negation is that it turns any short sentence to which one will assent into a sentence from which one will dissent and visa versa. That of conjunction is that it produces compounds to which (so long as the component sentences are short) one is prepared to assent always and only when one is prepared to assent to each component. That of alternation is similar with assent changed twice to dissent.[602]

Thus if the construction within the language being investigated fulfils these criteria, one can treat them as construct ions similar to the truth functions indicated.[603]

In this connection, the question arises whether one is imposing a particular logic upon the persons using the language being translated. What if their logic was quite different from the logic of English-speaking persons? Quine considers an extreme case, he writes: "let us suppose that certain natives are said to accept as true certain sentences translatable in the form '**p** and not **p**'."[604] Of course, as he indicates, this claim would be absurd under the criteria he has given. Quine's claim is that the best translation is one that imposes our logic on the language being translated. "Wanton translation can make native sound as queer as one pleases."[605]

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602 ***Ibid***., pp. 57-8. He restricts it to 'short sentences' here in order to avoid the situation where a subject gets confused by a more complex situation. He writes: "No limit is imposed on the lengths of component sentences to which negation, conjunction, or alteration may be applied; it is just that the test cases for first spotting such constructions in a strange language are cases with short components." (***Ibid***., p. 58.)

603 Quine writes: "Incidentally we can translate the idiom into English as 'not', 'and', or 'or' as the case may be, but only subject to sundry humdrum provisos, for it is well known that these three English words do not represent negation, conjunction, and alternation exactly and unambiguously." (***Ibid***., p. 58.)

604 ***Ibid***., p. 58.

605 ***Ibid***.

Of course, in the context of translation, Quine is right about this. Good translation does impose our logic upon the natives, for we want to have translations of native sentences that make sense in our language. Our language may not be equipped to deal with a peculiar native logic. So naturally in a translation context[606] the question of prelogicality must be begged. In giving a translation, we presume that what is being said is consistent.[607] The whole point of translation is to come up with a translation that not only captures the content of the sentence being translated, but makes sense in the translated version if it makes sense in the original version.

In translating (and other similar enterprises) we proceed in such a way as to preserve the usual roles of our logical connectives and tend to adjust our translation at other points in order to preserve these logical functions.[608] As Quine puts it so neatly: "The maxim of translation underlying all this is that assertations startlingly false on the face of them are likely to turn on hidden differences of

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[606] Quine points to other contexts where we try to preserve consistency. "Thus when to our querying of an English sentence an English speaker answers 'Yes and no', we assume that the queried sentence is meant differently in the affirmation and negation; this rather than that he would be so silly as to affirm and deny the same thing. Again, when someone espouses a logic whose laws are ostensibly contrary to our own, we are ready to speculate that he is just giving some familiar old vocables ('and', 'or', 'not', 'all', etc.) new meanings." (***Ibid***., p. 59.)

[607] Provided, of course, we are not translating a piece where the whole point of the piece is the pointing out of an inconsistency. Certainly in such a case, we may have to provide a translation of an inconsistency.

[608] Even in the case where we are dealing with the language of a system-builder who deliberately has altered his logic, we function in a similar way for translation purposes. Quine writes:

"Or consider the familiar remark that even the most audacious system-builder is bound by the law of contradiction. How is he really bound? If he were to accept contradiction, he would so readjust his logical laws as to insure distinctions of some sort; for the classical laws yield all sentences as consequences of any contradiction. But then we would proceed to reconstrue his heroically novel logic as a non-contradictory logic, perhaps even as familiar logic, in perverse notation." (***Ibid***., p. 59.)

language."[609] This, of course, does not preclude a particular culture from having and using a logic different from the logic that seems basic to our culture. Rather all Quine is saying is that in the context of setting up a translation scheme, we will tend to preserve our logic in order that the translations make sense to us. However, if it were to turn out that we could not make a proper translation in this fashion, then we would need to look further for the hidden differences of language. How these differences would be resolved in the end is difficult to say from a theoretical point of view. (It would depend on the particular situation). However, the attempt will likely try to provide the most sense for those who speak the language into which the native tongue is being translated.[610]

The important point to notice for purposes of our discussion is that logical connectives are learned from sentential contexts in a way similar to that in the case of 'bachelor' and 'unmarried man'. Detach our logical particles from these semantical involvements and we will have very little left.

Quine makes an attempt to deal with other more difficult logical functions in a similar semantical way. He first considers how we might deal with the categoricals A, E, I, 0 ('all are', 'none are', 'some are', 'some are not'). He tries to give a semantic criterion for A as follows:

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609 ***Ibid***., p. 59.

610 One can imagine similar difficulties with English speakers with radically different beliefs. Translation would be needed in these cases too, if we were somehow going to get behind their bizarre belief system. So this is probably not confined to translation between languages, but may likely apply to translation within a natural language. One thinks immediately of the translation from language of the younger generation to the language of the older generation.

> A semantic criterion for A perhaps suggests itself as follows: the compound commands assent (from a given speaker) if and only if the affirmative stimulus meaning (for him) of the first component is a subclass of the affirmative stimulus meaning of the second component and the negative stimulus meanings are conversely related.[611]

However, he claims that this whole idea is wrong for reasons similar to those which prevented us from using stimulus-synonymy of occasion sentences to get at the stimulus-synonymy of terms.[612] Quine's point is that just as terms were part of our special apparatus of objective reference, so too are categoricals. On the basis of stimulus meanings alone we were not able to distinguish between an affirmation of rabbits or of rabbit stages (something like identity was needed to get to the referent). The truth of categoricals also depends on objects picked out by a conceptual scheme. As we saw, these objects are not "uniquely determined by stimulus meanings."[613] According to Quine: "... the categoricals, like plural endings and identity, are part of our own special apparatus of objective reference."[614]

Quine concludes that:

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611 ***Ibid***., p. 60.

612 A less serious problem that Quine indicates arises when you consider problems that crop up over differences of stimulus meanings as in the case of 'Indian nickel' vs. 'Buffalo nickel'. Quine writes: "... but still the affirmative stimulus meaning of 'Indian nickel', for our novice anyway, has stimulus patterns in it that are not in the affirmative stimulus meaning of 'Buffalo nickel'. On this score the suggested semantic criterion is at odds with 'All Fs are Gs' in that it goes beyond extension." (***Ibid***., p. 60.)

613 ***Ibid***., p. 61.

614 ***Ibid***.

> Of what we think of as logic, the truth-functional part is the only part the recognition of which, in a foreign language, we seem to be able to pin down to behavioral criteria.[615]

As we indicated above, even this aspect is difficult to pin down by behavioral criteria. It was only in the context of translation where one is deliberately trying to map one language onto another that one imposes one's own logic on the other language in order that the translated passages make sense within one's own language. All this says is that in translating one tends to preserve one's logic and make adjustments elsewhere in order to get at the content of the translated sentence. This does not say that the truth-functional part of logic really is able to be pinned down by behavioral criteria. It only seems that way because of the procedure or methodology of translation. Basically, logic is very much a basic part of one's conceptual scheme[616], be it the truth-functional part, the categorical part, or what have you (certainly as we indicated in Chapter Two, quantification is very much a part of one's conceptual scheme.)

All this, of course, leaves the function of logic within a particular conceptual scheme very vague and in need of elaboration. We shall just make the point that logic is an integral part of a conceptual scheme and nothing shared outside of any particular conceptual scheme. This latter point is consistent with what the D-thesis is about, but one needs to spell out this relationship in greater detail.

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[615] ***Ibid***.

[616] This view is at odds with the view professed by Quine in ***Word and Object*** and indicates one point where the author wishes to depart from Quine's position.

The next aspect that Quine considers is the notion of "synonymous and analytic sentences."[617] He takes 'synonymous' to connote 'sameness of meaning' for both simple and complex expressions. He concentrates on what he calls the broader notion of synonymy of sentences, the notion that "the two sentences command assent concomitantly and dissent concomitantly, and this concomitance is due strictly to word usage rather than to how things happen in the world."[618]

In the case of occasion sentences, Quine writes, "the envisaged notion of synonymy is pretty well realized in intrasubjective stimulus synonymy, especially as socialized."[619] This is clearly so because of the way that Quine has defined socially stimulus-synonymous (as that which comes out as stimulus-synonymous for "each individual speaker almost without exception."[620]) Some standing sentences may also be well served by the notion of stimulus synonymy. Quine writes: "When the sentences are standing sentences, which, like 'The Times has come', closely resemble occasion sentences in the variability of assent and dissent, stimulus synonymy still does pretty well."[621] You will recall that the distinction between standing sentences and occasion sentences was one of degree,

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[617] Section 14 of Quine's Chapter Two is entitled "Synonymous and Analytic Sentences".

[618] ***Ibid***., p. 62. The narrower sense of synonymy which Quine says can be defined on the basis of the broad one" (***Ibid***., p. 62.) is like Carnap's intensional isomorphism involving certain part-by-part correspondence of the sentences concerned." (***Ibid***., p. 62.) Quine describes how the one can be defined in terms of the other as follows: "Synonymy of parts is defined by appeal to analogy of roles in synonymous wholes; then synonymy in the narrower sense is defined for the wholes by appeal to synonymy of homologous parts." (***Ibid***., p. 62.)

[619] ***Ibid***., p. 62.

[620] ***Ibid***., p. 55.

[621] ***Ibid***., p. 63.

rather than one of kind, so in this case Quine is referring to standing sentences that approach being occasion sentences. Assent and dissent for occasion sentences varies a great deal from moment to moment because of the variations in the situation. A standing sentence like 'The Times has come' varies daily, perhaps, whereas a sentence like 'Red' may vary from moment to moment according to the object being pointed to or indicated in some way or according to lighting conditions, etc. This suggests that by extending the modulus of stimulation stimulus synonymy can serve most standing sentences. However, this ploy does not work for if the modulus of stimulation is too long other factors intervene. As Quine puts it:

> The trouble is that there is no telling what to expect under fairly fantastic stimulation sequences of such duration. The subject might revise his theories in unforeseeable ways that would be claimed to change meanings of words. There is no reason to expect the concomitances of sentences under such circumstances to reflect present sameness of meaning in any plausible sense. Lengthening the modulus enriches stimulus meanings and tightens stimulus synonymy only as it diminishes scrutability of stimulus synonymy.[622]

(In the above paragraph, Quine is talking of a modulus of one month.) One can imagine testing the two sentences 'This month is December', and 'Christmas is celebrated this month' under the modulus of one month. Individuals will assent and dissent concomitantly. Lengthening the modulus of stimulation will certainly not do.

According to Quine: "Stimulus synonymy, on an optimum modulus, is an approximation to what philosophers loosely call sameness of confirming experiences and of disconfirming

[622] ***Ibid***.

experiences."[623] Sameness of confirming and disconfirming experiences does not provide a means of getting at sameness of meaning except in the case of occasion sentences. This is because not all sentences are directly related to experience. As Quine says:

> If the business of a sentence can be exhausted by an account of the experiences that would confirm or disconfirm it as an isolated sentence in its own right, then the sentence is substantially an occasion sentence. The significant trait of other sentences is that experience is relevant to them largely in indirect was, through the mediation of associated sentences.[624]

To give an account of how other sentences are connected to experience, we really need to have an account of how this mediation through associated sentences works. This is where the D-thesis comes in. It tells us that there are many ways of seeing these connections. Each of these ways differ, for example, in the way they alter a theory to take account of a recalcitrant experience. Quine writes:

> Alternatives emerge: experiences call for changing a theory, but do not indicate just where and how. Any of various systematic changes can accommodate the recalcitrant datum, and all the sentences affected by any of those possible alternative readjustments would evidently have to count as disconfirmed by that datum indiscriminately or not at all. Yet the sentences can be quite unlike with respect to content, intuitively speaking, or role in the containing theory.[625]

Certainly one cannot make the readjustments indiscriminantly. There are some readjustments that take account of the recalcitrant experience and some that do not. What we need from Quine is some way of distinguishing the most effective readjustments from among those that do take account

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[623] ***Ibid***.

[624] ***Ibid***., p. 64.

[625] ***Ibid***.

of the recalcitrant experience. Intuitively, we feel that there are some effective readjustments that are better than others. What we had hoped to find in ***Word and Object*** was some way of deciding which ones were better. It would appear that Quine does not wish to draw out his 'Two Dogmas' metaphor this far. For this thesis to be of any use it would need to have some account of how sentences other than occasion sentences are indirectly related to experience. What is the internal fabric of language like? What is its basic weave pattern? Quine seems to leave these important questions hanging.

He discusses the attempt by Grice and Strawson to get around this difficulty: "by defining S_1 and S_2 as synonymous when, for every assumption as to the truth values of other sentences, the same experiences confirm (and disconfirm) S_1 on that assumption as confirm (and disconfirm) S_2 on that assumption."[626] Essentially, then, their ploy is to hold the system constant while testing the particular sentences. So naturally their notion of synonymy is a notion relative to a particular system of sentences. Quine tidies up their statement as follows: "So S_1 and S_2 are defined to be synonymous when, for every S, the same experiences confirm (and disconfirm) S_1 on the hypothesis S as confirm (and disconfirm) S_2 on S."[627] Since there seems to be intimate relationship between stimulus meaning and confirmation, Quine wonders whether it, too, can be relativized to an hypothesis S. Of course, Quine thinks it can be; "for confirmation or disconfirmation of S_1 on S is presumably confirmation or disconfirmation of the conditional sentence consisting of S as antecedent and S_1 as consequent."[628]

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[626] ***Ibid***.

[627] ***Ibid***. Quine is here taking S to be "the logical conjunction of these "other sentences" in question or their negations." (***Ibid***.) In this way the phrase "every assumption as to the truth values of other sentences" can be replaced by the phrase "every S."

[628] ***Ibid***.

Quine's definition becomes "S_1 and S_2 are synonymous if for every S the conditional compound of S and S_1 and that of S and S_2 are stimulus-synonymous."[629] The problem is that this definition cannot provide a definition for synonymy stronger than stimulus-synonymy. And the notion of stimulus-synonymy was found to be adequate only for occasion sentences and possibly even some standing sentences that bordered on being occasion sentences.

Quine next recalls the relationship between "intrasubjective sentence synonymy"[630] and analyticity. In Chapter Two we saw that these two notions are interdefinable. He repeats the definitions in ***Word and Object***:

> ... sentences are synonymous if and only if their biconditional (formed by joining them with 'if and only if' is analytic, and a sentence is analytic if and only if synonymous with self conditionals ('if then p').[631]

In his earlier writings, Quine also pointed out the relationship between analyticity and necessity. Here he ponders the relationship between analytic truths, **a priori** truths and necessary truths. The traditional questions are whether all a priori truths are analytic (the question of whether there is a synthetic **a priori**) and whether all necessary truths are **a priori**? Some philosophers who have identified the three, analytic, **a priori**, and necessary, have (certainly, under the promptings of Quine's "Two Dogmas...") regarded the analytic sentences as "those that we are prepared to affirm come what

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629 ***Ibid***., p. 65.

630 ***Ibid***.

631 ***Ibid***.

may."[632] Quine's response to this is that the definition "comes to naught unless we independently circumscribe the 'what may'."[633] One way that he suggests is to treat 'come what may' as 'come what stimulation may',[634] Under this construal, we have what Quine calls **stimulus analyticity**.[635] A sentence is "**stimulus-analytic** for a subject if he would assent to it, or nothing, after every stimulation (within the modulus)."[636] Thus a stimulus-analytic sentence is one that an individual will assent to regardless of experience. Quine even suggests socializing stimulus-analytic in the same way that he socialized stimulus synonymy. Thus sentences are socially stimulus-analytic if they are "stimulus-analytic for almost everybody."[637] The problem is that this notion of analyticity does not do the job, for such an obvious empirical truth as 'There have been black dogs'[638] will come out socially stimulus-analytic as well as sentences that are intuitively agreed to be analytic such as '2 + 2 = 4' and 'No bachelor is married'.[639]

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[632] ***Ibid***., p. 66.

[633] ***Ibid***.

[634] ***Ibid***.

[635] The same relationships of interdefinability hold between stimulus analyticity and stimulus synonymy as hold between analyticity and synonymy. (***Ibid***., p. 65.)

[636] ***Ibid***., p. 55.

[637] ***Ibid***., p. 66.

[638] Quine's example, ***Ibid***., p. 66.

[639] Also Quine's example.

Quine speculates on the source of our intuitions of what is analytic. He feels that "the notion of "assent come what may" gives no fair hint of the intuition involved."[640] He falls back on the notion of communication. Just as in the case of translating a previously unknown foreign language we tended to preserve our own logic in order that that sentence translated made sense in our language, and so facilitated communication, so also in the case of analyticity, certain sentences are preserved because they are basic to communication. He writes:

> ... dropping a logical law disrupts a pattern on which the communicative use of a logical particle heavily depends. Much the same applies to '2+ 2 =4', and even to 'The parts of the parts of a thing are parts of the thing'. The key words here have countless further contexts to anchor their usage, but somehow we feel that if our interlocutor will not agree with us on these platitudes there is no depending on him in most of the further contexts containing the terms in question.[641]

The point is that communication demands a certain amount of conformity and it is the analytic sentences that form the basis of communication. Of course, this is quite vague (as Quine admits), however, this **mechanism of analyticity intuitions**, as Quine calls them,[642] is fertile ground for the development of the D-theoretic point of view. The point of the D-thesis is that under certain conditions (as yet unspecified) one ought to be able to challenge these analyticity intuitions.[643]

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640 ***Ibid***.

641 ***Ibid***., p. 67.

642 ***Ibid***.

643 These, however, do not have the firmness needed for the basis of a distinction between analytic truths and synthetic truths. As Quine puts it:

"The intuitions are blameless in their way, but it would be a mistake to look to them for a sweeping

This short section on analyticity in ***Word and Object*** does enlarge slightly the concerns discussed in "Two Dogmas...", but it certainly does not adequately fill out the metaphor of "Two Dogmas...". Unfortunately for the project of this thesis, this is all the filling out that Quine does in ***Word and Object***.

To summarize briefly, Quine has certainly grounded language in experience by means of his notion of stimulus meaning. He has begun a classification of sentences according to their empirical content. Occasion sentences are the most empirical since there is a direct correspondence between stimulation and an individual's response to these sentences. However, these sentences are not purely empirical, since they, too, suffer the effects of prior stimulation and collateral information. Standing sentences were less empirical and depended more for their meaning on their relationships within language. Their connection to the world was more indirect through the medium of associated sentences. Quine, however, does not fill out the relationship involved here. An adequate filling-out of the metaphor of "Two Dogmas..." requires that these relationships be exposed. Quine discovers that both terms and certain logical connectives (although this writer would say all logical functions) are part of the conceptual apparatus of a particular language. Finally in his discussion of the interdefinable notions of synonymy and analyticity, Quine vaguely suggests the relationship between analyticity intuitions and communication.

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epistemological dichotomy between analytic truths as by-products of language and synthetic truths as reports on the world. I suspect that the notion of such a dichotomy only encourages confused impressions of how language relates to the world." (***Ibid***., p. 67.)

The remainder of ***Word and Object*** can be skipped over for our purposes[644], since it does not pertain directly to the subject matter of this dissertation, but is rather an elaboration of Quine's theory of reference, and his search for an austere canonical form for the system of the world.[645]

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644 At this point, I want to skip over a great deal of ***Word and Object***. Although the remainder is related indirectly to the concerns of the thesis, it is not directly relevant to the matters being considered. A good deal of the content of the later portions of ***Word and Object*** was introduced by Quine in his earlier writings, and he is working out the details of these earlier suggestions in the latter parts of ***Word and Object***. Some of these topics were mentioned in passing in our second chapter. Some of the topics considered are: singular and general terms, prediction, ostension, identity, abstract terms, vagueness, ambiguity of syntax, ambiguity of structure, ambiguity of scope, referential opacity, substitutivity of identity, referential transparency, simplification of theory, paraphrase into logical symbols, maxim of shallow analysis, quantifiers, classes, intensions, tense, nominalism, extensionalism, absolutism, modality, to name a few of the considerations taken up. These are fascinating sections for any logician. A good number of the issues are working out of certain preferences of Quine, such as his use of the theory of quantification to get at ontology. A good number of the concerns are of especial importance to anyone studying the system of objective reference in our conceptual scheme. These points are peculiar to our quantificational way of analysing things. (Quine, we saw in Chapter Two, pointed out that there are other ways as good as quantification).

645 Quine's austere canonical form for the system of the world needs only three basic constructions: prediction, universal quantification, and the truth functions, as well as the ultimate components of variables and general terms. (***Word and Object***, p. 228.) Quine, however, does not claim that all the idioms, like indicator words, subjunctive conditionals, propositional attitudes, modalities, intensional abstraction, etc., that he dispenses with in the middle chapters of ***Word and Object*** are not needed. They do help to simplify matters. All he is saying is that they can be replaced (at some cost). He writes:

> "Not that the idioms thus renounced are supposed to be unneeded in the market place or in the laboratory. Not that indicator words and subjunctive conditionals are supposed to be unneeded in teaching the very terms - 'soluble', 'Greenwich', 'A.D.', 'Polaris' - on which the canonical formulations may proceed. The doctrine is only that such a canonical idiom can be abstracted and then adhered to in the statement of one's scientific theory. The doctrine is that all traits of reality worthy of the name can be set down in an idiom of this austere form if in any idiom." (***Ibid***., p. 228.)

> Essentially, Quine is presenting a relative doctrine of categories. He writes:

> "Of itself it sets no limits to the vocabulary of unanalyzed general terms admissable to science. But it sets limits to the ways of deriving complex predicates, complex conditions or open sentences, from those undictated components. It is a doctrine that limits what can be said of things to (a) such "prime traits" or general terms as may bc expressly admitted severally on merits beyond this doctrine's purely relativistic concerns, and (b) such

"derivative traits" as can be formulated in those primary terms with help of prediction, quantification and truth functions alone. It delimits what counts as scientifically admissible construction, and declares that what ever is not thus constructible from given terms must either be conceded the status of one more irreducibly given term or eschewed. The doctrine is philosophical in its breadth however continuous with science in its motivation. (***Ibid***., pp. 228-9.)

EXHIBIT ONE

Emerson Hall
Harvard University
Cambridge 38, Massachusetts
October 2, 1962

Professor Harry Frankfurt
Department of Philosophy
The Ohio State University
Columbus 10, Ohio

Dear Professor Frankfurt:

I have just finished a book on set theory, and have turned at last to clearing up a distressing interim accumulation of neglected reprints and mimeograms. I am shocked now to find among them something older than the rest, and an original typescript at that. It is your "Meaning, truth, and pragmatism" of March 1960. True, you said I could keep it; otherwise I'd have acted somewhat promptly; still, I had not meant to delay thus indefinitely.

Ironically, my Word and Object came out about simultaneously: March 1960. As you may meanwhile have noticed, this book is largely concerned with expanding, supplementing, and improving the doctrine that was so inadequately sketched in those last four pages of "Two dogmas."

A central point of your criticism was that I leave myself no remnant of empiricism. In terms of Word and Object, Chapter II, my answer is that the empirical component is provided by the stimulus meaning, which is overwhelming in observation sentences and not inconsiderable in many other sentences. This doctrine is the filling in of what was sc briefly and metaphorically hinted in terms of "distance from periphery" in "Two dogmas."

Also, what certainly is vital, there are in Word and Object disavowals of a too monolithic holism; e.g., p. 13n. Critics of those last pages of "Two dogmas" on this score are certainly not to be blamed. On the other hand the holism still seems right to me in essential respects, and it is what makes for the "indeterminacy of translation" urged in Word and Object (and foreshadowed in From a Logical Point of View, Essay 3).

Another difficulty that you raised was that since I referred all meaning to the whole system, I retained no way of making sense of sameness of statements. In this connection I would clarify first a minor point of terminology: in my own writings, early and late, 'statement' has always referred to linguistic forms and not to their meanings. This may have thrown you off in specific passages, but your general point remains important, and it is one that I recognize and discuss in Word and Object, top p. 24 and elsewhere.

I am sorry you did not then have before you a statement of my developing views that was more worthy of your mettle. This mettle I find formidable, and I do not flatter myself that Word and Object is proof against it; but still I should there expect somewhat less the impression of a steel trap on a butter ball.

And I am doubly sorry for the oversight that has delayed this letter a couple of years longer than mere preoccupation could have done.

Sincerely yours,

W. V. Quine

W. V. Quine
Professor of Philosophy

WVQ/dlr

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